BULLETIN

OF

THE JOHNS HOPKINS HOSPITAL.

Vol. VIII.—No. 72.] BALTIMORE, MARCH, 1897. [Price, 15 Cents.

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IS MALARIA A WATER-BORNE DISEASE?*

By Rupert Norton, M. D., *Washington, D. C.*

There are several points in the consideration of this question which make an exact answer extremely difficult. First and foremost stands the fact that the malarial organisms, those forms producing malarial fever in mankind, have not been recognized outside the blood of human beings. Other similar types or species of organisms have been found in the blood of birds, frogs, etc., but these do not seem to be of the same species as that which produces the fever in human beings. Thus at the outset we are hindered in the investigation of this question by not being able to find and study the life-history of these organisms in any extra-corporeal form. Another difficulty, and not a minor one, is the occurrence of malarial fevers and typhoid fever in the same places at the same time of the year, oftentimes with very similar types of fever. And again it is generally supposed that the malarial organisms exist in the soil, in damp soils, and so possibly in actual water, whether stagnant or running, and as the germs of typhoid fever exist in water, and we know that typhoid fever may be communicated by drinking-water, the belief is a common one that malarial fevers may be induced in the same way.

Proof that malaria is a water-borne disease rests only on evidence that after drinking certain waters people sometimes suffer from so-called malarial chills or fever. It may be shown that they have the malarial organism in their blood, but it cannot as yet be proved that the water they drank contained this organism, so that the chain of evidence is incomplete. We believe there is almost no one who denies the possibility and even probability that malaria is ordinarily an air-borne infection. This makes the proof of the water-borne theory still more difficult, for in every case where the air-borne theory might be tenable as against the water-borne, proof must be brought that the patient was not infected through the air—an almost impossible task. Malaria is almost as wide-spread a disease as typhoid, and yet does not occur in epidemics. No group of cases of malarial fever on land has ever been traced to a single source of infection, as has been done so frequently in epidemics of several of the infectious diseases, especially of typhoid fever. We do not find groups of patients with malarial fevers in towns or elsewhere whose infection can be traced to a single supply of water or milk. We see cases of malarial fever springing up sporadically, apparently having no direct connection with each other, except through the medium of the air: the very name of the disease throws light upon the common idea of its origin. Popular beliefs are against, rather than in favor of the water-borne theory; for example, the general conviction in Rome of the danger of being out after sunset in the Campagna for fear of "catching" malaria, or rather being "caught" by it, and the belief in the security derived from living in the second story of houses in malarious countries, where the dwellers in the first story suffer severely. A similar thought existed among

*Read before the Johns Hopkins Hospital Medical Society, Nov. 16, 1896.

the laborers on the Panama Canal, and they, to escape the effluvia arising at night from the soil, slept in the trees or built houses on stakes high above the ground level. This, of course, is no positive proof against the water-borne theory, but shows what the feeling has been amongst many people as to the larger share which air, rather than water, plays in the etiology of this disease.

To show the difficulty in properly differentiating typhoid and malarial fever, we need only to look at a few statistics and remarks made by men of repute who have written about malaria. Chapin,[1] in 1884, reviewing all the cases that had occurred in New England since 1861, gives the following data. He reports the deaths due to malaria and typhoid fever in Massachusetts, Connecticut, and Rhode Island, and states that from 1878 to 1882, in Connecticut 1217 persons died of malaria and only 1242 from typhoid. In Massachusetts during the same time there were only 159 deaths from malaria to 4349 from typhoid, and in Rhode Island 24 to 794. The table from Connecticut is an interesting one to look at:

	Deaths from Malaria.	From Typhoid.
1876	22	327
1877	73	321
1878	143	260
1879	198	159
1880	265	242
1881	357	257
1882	264	324

As seen from these figures, during the years 1879, 1880 and 1881 there were fewer deaths from typhoid than from malaria. It is not worth while to discuss them. It may be said from what we now know of malaria that the records upon which they are based are absolutely untrustworthy.

Griswold[2] says: "For the main interest of this article, I accept the term 'malaria' as implying that undetermined potency producing the general class of fevers of which intermittent is the most pronounced type, and in which class is included various other diseases that have been very generally regarded as the results of paludal poisoning. In the absence of such specific knowledge as would enable us to give the class a more definite and satisfactory designation, the term answers as well as would any other equally arbitrary." This was written after Laveran's discoveries. Here are two extracts from papers on malaria as seen in New England. Greenleaf[4] states that malaria may be due to drinking water or to food contaminated by the hands, or to air; but he believes that "intermittent fever is a disease always originating locally, thus excluding as etiological factors all causes of a general character." Certainly air and water are causes of a general character, especially the former. Green[5] believes that possibly in the cases reported by him, the water of wells was poisoned by backing up of water, due to a dam or causeway which prevented natural drainage of the adjacent soil.

It is impossible to draw correct conclusions as to the etiology of malarial fevers from such general statements. Finally in the Fourth Annual Report of State Board of Health, New York, 1883, on Malarial Fever at Lakeville, Washington county, we read as follows:

"Malarial fever has also appeared at the house of Mr. Donald Reid, about one-third of a mile from Lake Cossayuna, up the Summit pond brook at a height of one hundred feet above the lake. No other cases of fever were reported in this direction. An examination of the premises of Mr. Reid showed that his house stands upon rock, covered with a comparatively thin layer of clay, and that while the roads and ground a short distance away from the house were dry and dusty, the clay around this dwelling was saturated with water. This water comes from a spring north of the house. The water is brought from the spring to the barnyard through a pipe, and empties into an open trough, the overflow of which saturated the surrounding ground. The surface of the ground here is twelve feet higher than that about the dwelling. The upper stratum is a loose soil some two feet deep, through this the water percolates until it reaches the clay over the rock, along which it readily finds its way down the steep slope to the house and saturates the clay about the dwelling. Water is also brought directly from the spring to the house and empties into a trough on the east side of the dwelling. The overflow of this trough is allowed to saturate the ground within ten feet of the eastern wall of the house. From these facts it appears that saturation of the soil about this dwelling, for which the owner is entirely responsible, is a sufficient cause for malarial fever in a region bordering upon such extensive swamps."

There can be but little doubt that these cases were not malaria, but typhoid; certainly the conditions existing were just such as to produce typhoid. Such unreliable statements as those quoted have led many who have not had the opportunity to investigate the question for themselves to believe that malaria was a water-borne disease.

It is a curious fact that almost all observers who want to prove that malaria may be due to drinking water refer to the cases of illness which broke out on the now famous ship Argo. The *pros* and *cons* of the evidence that these cases were or were not malaria have frequently been discussed and there is no general agreement. It seems quite fair, however and more than probable, to conclude that these cases were not malaria. At any rate the burden of proof lies with those who maintain that the cases were malaria, and the proof they offer is unsatisfactory. If these were malaria, then the water-borne theory needs no further demonstration; but for those who doubt, new proof must be furnished. The story related by Boudin[3a] is as follows. I translate freely:

"In the month of July, 1834, the Sardinian ship Argo sailed from Bone (Algeria) with 120 soldiers in good health, and arrived at Marseilles. Thirteen men had died during this short voyage and had been buried at sea; 90 men were brought to the hospital in Marseilles, showing pronounced ("les moins équivoques") symptoms of paludal intoxication, in all its forms and types; in some cases the disease was most severe, or if one prefers to say so, showed the greatest degree of perniciousness. Whereas the soldiers were attacked by choleriform, epileptic, comatose, tetanic and other types of fever, which yielded as though by enchantment to large doses of quinine, the sailors showed a marked contrast in their perfect health. Now what could be the cause of such a difference in individuals who were apparently, at least, subjected to identical conditions?

An official inquiry, of which I had the direction, gave me the most complete information. The investigation showed that if the sailors had kept their good health they owed it to the purity of the drinking water, of which they had a special supply, while the soldiers were forced to drink water drawn from a well in a marshy spot near Bone, which water had been taken on board just on the eve of departure. The soldiers who escaped this poisoning were those who, having saved a little money, were able to buy water from the Sardinian sailors. This fact demonstrates in a peremptory manner how paludal matter absorbed in a liquid form from the gastro-intestinal surface, as by the lungs in a gaseous form, causes intoxication."* In another place the author himself admits that his evidence is not conclusive. He says: "Le fait suivant, dont nous avons été témoin au lazaret de Marseille, in 1834, pourra paraître concluant au premier abord, et pourtant, en y regardant de près, on est contrait de rester dans le doute."

It is strange that nothing is said in this story as to the length of the voyage, nor whether the sailors had been allowed to land at Bone. Algeria is a malarious country, and if the cases were malaria, the soldiers may have been infected on land, and the drinking water may have only aggravated the sickness. In fact there are many points in the story which need elucidation and without which the story is scientifically unreliable.

Oswald Baker[3] relates some cases of a similar sort, but here, too, his proof is insufficient, not so convincing even as that of the Argo. He attributes the cases of illness, which are not described, to the drinking water on board a vessel. Kershner[4] relates the case of a ship in which malaria (?) broke out, and where all the evidence went to prove that it was not due to drinking water. The author does not believe in the water-borne theory. The evidence is not clear as to what the illness really was. These are all the cases that I have found to prove or not to prove the development of malaria on shipboard as due to drinking water. If one case could be definitely shown to have occurred in this way, far from land, where the possibility of an air infection would be reduced to a minimum, the case, even then, would not be proven, for we know that the case might be one of latent infection, developing long after a stay in a malarious country.

On the origin of malaria we find many articles by medical men in various countries, and it will be better to examine these systematically, and thus see what proof has been brought forward to confirm the water-borne theory. Let us first look at some of the Reports of the Surgeon-General of the United States Army. In the report for 1889, pp. 32-33, it is stated:

"Much of this improvement has been attributed, and I believe correctly so, to the introduction of wholesome water supplies. In a recent communication, Col. Charles Page, Assistant Surgeon-General, makes an exception of Fort Sill, Indian Territory, in this connection, claiming that the diminished malarial sick list which followed the introduction of a good water supply cannot be referred to the water, because there was a diminution of malarial diseases at Fort Supply, which had no new water supply, as well as Fort Sill, for several years after the introduction of the water supply at the latter post. He attributes the improvement to some general condition which affected Fort Supply as well as Fort Sill, and considers, therefore, that the theory of a water propagation of malarial fever does not always hold good. He says:

'An examination of the consolidated reports of the medical department of Fort Sill for several years will convince one that the supply of spring water did not diminish the ratio of malarial disease materially. The diarrhœas diminished and the general health of the command was improved, most probably by the beneficial effects of a full supply of good water for drinking and bathing.'

"Here is an example of a garrison suffering from malarial poisoning supposed to be contained in the drinking water, which continued to feel the effects of malaria although the supply of water was changed from that of a doubtful character to one absolutely pure, being filtered for miles through sand, gravel, and lime-stone rock. It is apparent that the poison producing malarial diseases must be sought for in another source than in the drinking water. It is there at Fort Sill in the heat, moisture and soil which produce malarial fevers anywhere, or furnish good culture material for the bacteria that cause it. In New Mexico, where the climate is dry, though hot, malarial fevers do not exist; but turn the waters from the rivers upon the soil by means of acéquias, and malarial fevers soon abound, regardless of the source of the water used for drinking. It will be observed that for several years before the improvement of the water supply at Fort Sill the ratio of malarial diseases was smaller than after the development of the spring which gave the garrison a pure filtered water. . . . In 1881 the ratio was remarkably small, being but 59 per 1000 of mean strength; surely that could not be accounted for by any improved source of the drinking water. There may have been some meteorological cause; we know that in different years there is a marked variation in the prevalence of malarial diseases. . . . The diminution of the cases of malarial diseases at Fort Sill after 1884, when it appears that the greatest number prevailed, must be considered a sequence and not a consequence of the development of the spring of excellent drinking water. The ratio of malarial diseases at Fort Supply for the years 1884, 1885 and 1886 shows a decrease of sickness after 1884. Fort Supply, where the water supply was not improved, like Fort Sill, has a reputation of being malarious. The water supply of this post has recently been materially improved . . . but it remains to be seen if the ratio of malarial fevers will be diminished."

In the report for 1890 it is stated: "Relatively fewer of the colored troops were attacked, but the proportion of remittents to intermittents was greater among them, and the average duration of the remittent was longer—34.8 days for the colored, and only 21.2 days for the white men—an illustration of a well established fact, that when the negro becomes seriously ill he does not rally so readily as the Caucasian."

*"I have said above that the patients showed signs of paludal intoxication in all its types; actually and for the first time at Marseilles, I observed cases of continued fevers, although quinine was administered ("des fièvres continues à quinquina"). Now my investigation showed that the patients with continued fevers were precisely those who appeared to be the most saturated with the marshy water."

The duration of the sickness in these cases is much against any considerable number of them having been malaria, and the use of the words "remittents" and "intermittents" adds to our difficulty in determining the etiology.

In 1890, Fort Brown, Texas, was supplied with distilled water, and the improvement in the general condition of the post after that year is attributed entirely to this factor.

The following quotation is found in the report for 1894, p. 52: Capt. W. Fitzhugh Carter, the post surgeon at Fort Sill, Oklahoma, writing on malarial diseases, says: "There is almost conclusive evidence that the water of the streams in this section of the country is a concentrated malarial poison, and at certain times of the year will give rise to malarial fevers in a large percentage of those who drink it," and he looks to the use of distilled water for the same beneficial results that have already been obtained at Fort Brown, Texas. But in the report for 1895, p. 41, we find that Fort Brown was the fourth army post having highest admission rate for malarial diseases. Washington Barracks, D. C., and Fort Meyer, Va., just across the river from Washington, came first and second in this list; both these places are well-known hotbeds of malaria. The third in rank was Fort Hamilton, in New York Bay, where it is more than probable that the percentage of cases attributed to malaria is too large. From this we do not see that there could have been any great improvement in the condition of Fort Brown, as it never ranked first on the list. The purer water supply does not seem to have reduced the cases of malaria very greatly. On the same page of the report for 1895 we find that "nearly one-fourth of the malarial cases of the army were remittent, with an average duration of 21.5 days; the average of the intermittents was 5.75 days"—a long period for remittent, if by that we are meant to understand malarial fevers, and closely corresponding to the length of time usually given to typhoid. A supply of water supposed to be quite pure has not modified to an appreciable extent the number of cases occurring at the two army posts in Washington. Dr. Charles Smart, of the U. S. A., is an ardent supporter of the water-borne theory, but he has brought forward no convincing evidence of the truth of this theory. He believes surface waters, when used for drinking purposes, to be more dangerous than water from deep wells.

The best, and a very interesting paper that has appeared on malarial fever in these reports is by Dr. Walter Reed in the number issued this year. He has paid special attention to the cases of fever at Washington Barracks and Fort Meyer, and has examined the blood of many patients. Dr. Reed does not believe in the water-borne theory. He tells me that "the water supply at Washington Barracks has not been changed; it has always been the same as that of the city. The supply at Fort Meyer was changed in November, 1894, from well water to Potomac river water. The following year the men and officers drank, as a rule, filtered and boiled Potomac river water, with the result that there were more cases of malarial fever in 1895 at Fort Meyer than during any one of the previous 23 years. It was also higher at the Barracks in 1895 than during any of the previous 23 years." There is much malarial fever in the city of Washington, and it is really endemic along the river flats, but has no apparent connection with the drinking water of the city, since otherwise the people in the upper part of the city should naturally suffer as much, which they do not. The reports from the army posts are hard to analyze, since the officers in charge are frequently changed, and their personal views as to malaria and typhoid fever vary materially. But, as yet, all the evidence coming from the army posts is rather against than for the water-borne theory.

Through the kindness of Dr. G. M. Sternberg, Surgeon-General of the U. S. Army, I have received to-day proof-sheets of an address lately delivered by him in New York. In this he says: The facts which have been developed with reference to the transmission of cholera and typhoid fever, considered in connection with numerous recorded observations relating to the supposed development of malarial fevers as a result of drinking surface waters, have led some authors to the conclusion that malarial infection, also, commonly results from the introduction of the malarial parasite in this way. Without denying that this may occur, I am disposed to believe that much of the evidence which has been advanced in favor of this view is unreliable. In many cases the so-called malarial fevers which have been traced to the use of surface water from wells, streams, etc., have been of a "continued type" and not controlled by quinine. In the absence of positive evidence of the presence of the malarial parasite in the blood, continued or remittent fevers which resist the specific action of quinine cannot, in our opinion, be properly classed with the malarial fevers. If not due to infection by the typhoid bacillus, they are, at least, more nearly allied to typhoid than to the typical malarial fevers."

A large number of general practitioners throughout the United States believe firmly in the water-borne theory, among them Zwisohn,[7] West,[8] Waggener,[9] and Hyland,[10] but the evidence they show to support their view is most incomplete.

Daly[11] wrote a long article attempting to prove this theory, and thought that vegetables washed in marshy water might produce malaria, but his paper is unconvincing. This called forth other articles by Norbury,[12] Jones,[13] and Cadwallader;[14] the first two mentioned agree with Daly, but Norbury gives an account of an epidemic which might have been typhoid, and Jones' paper is still less satisfactory than Norbury's. Cadwallader does not believe in the water-borne theory, but thinks the home of the plasmodia is in the sap of vegetable life rich in sugar, and thinks there is danger of infection in eating maple sugar and sorghum molasses. This startling theory is balanced by an equally strange one put forth by Newton,[15] who thinks "the consumption of water-melons seemed to be provocative of malarial fever." He believes both the air- and water-borne theories have been proven, but most of his evidence is taken from the works of other men, some of whom I have mentioned, and his paper adds nothing material to strengthen the water-borne theory. Hurley,[16] like Daly, attributes malaria to drinking water, "though fruits and vegetables eaten raw may carry it." He does not believe it enters through the lungs. His proof is based on some cases, where people using boiled water were not attacked by some vague symptoms, which those who did not purify their water in this way, suffered from.

Lewis[17] believes "water to be one of the chief avenues, if not

the most important, by which malaria gets into the human system." His ideas are based on inquiries as to the origin of malaria sent to various doctors and laymen in the South; the replies of these gentlemen are almost all of them in favor of the water-borne theory; but the cases given are not described with sufficient detail to rule out typhoid in many instances, nor can infection through the air be eliminated, nor the possibility that the disease was acquired in some other locality than that where the patient was living when found sick. Many of the reports are founded on the disappearance of illness and fewer deaths in families when the supply of water was changed; the source of the drinking water often stood below the level of the houses and could easily have been polluted; many cases of illness were said to be the result of drinking cistern water. The health of the people was improved by a change in position of a well, or from driving deep wells, and not depending on surface water for drinking purposes. Such evidence cannot be accepted as satisfactory proof of malaria being the result of drinking impure water. Cowan[17] reports a number of cases which he believes support the water-borne theory, but his ideas on the subject of malaria are so opposed to the exact knowledge we have at present in regard to that disease that his evidence may be fairly thrown aside as valueless. He says: "We shall, however, assert as true that the malignant types of malarial fever, and in fact almost all forms of malarial fever, are indebted to the water we drink, and not to atmospheric conditions, for the etiological factor in the transmission or reception of the pathogenic material which produces malarial fever. . . . In contradistinction, allow me to say that water in globule never produces malaria, but becomes an absorber and retainer of this pathogenic material, and may become loaded with this poison almost to saturation. Trees, wells, springs, creeks and branches may become so loaded with this poison that a single glass of water may contain pathogenic material sufficient to produce a case of intermittent fever. Without entering further into the manner of its formation or the power of water in globule to absorb or retain this pathogenic factor and disease producer, we will endeavor to give proof that the water we drink is the great factor and principal source of malarial fever in all its forms, but especially in the malignant types."

If the cases reported by Quine[18] could be accepted without hesitation, their evidence might be looked upon as strongly in favor of the water-borne theory, but many of the facts given are so entirely contradictory of what has been proved as regards the effect of quinine on the malarial parasite that we cannot accept them—we feel sure that some mistake was made in the diagnosis. We quote the following cases in behalf of the stand we take:

Case 4. "Miss H., aged 16; has lived in Chicago many years in a good house and neighborhood. No excavations near. April, 1894, she began to be sick gradually with a continued type of fever marked by irregular fluctuations, which was labeled by the attending physician, Dr. Samuel Cole, 'typhoid fever with malarial features.' The fever continued about four weeks, and then, after an interval of normal temperature, lasting a few days, there was a relapse. The patient had taken quinine freely from the beginning. Upon the basis of two examinations made by myself during the month of May, 1894, the diagnosis of the family physician was sustained. Later the blood of the patient was examined by Dr. F. S. Johnson and the fact of malarial infection, previously assumed to exist, was demonstrated. Quinine was administered with great freedom by the mouth, rectum and hypodermic method, and arsenic and iron were also pressed systematically, but without affecting the course of the disease. The patient was seen at this time by Dr. N. S. Davis. Her illness continued over four months, uninfluenced by treatment to an important extent, and finally came to be attended with acute pulmonary tuberculosis. Death occurred five or six weeks later. *The blood of the patient crowded with plasmodia to the last.*" [The italics are mine.—R. N.]

Such resistance on the part of the plasmodia is unknown, especially when persisting in this manner for weeks. It is contrary to all the best evidence we have. Equally doubtful are the following cases reported by him.

Case 5. A girl with an alveolar abscess was thought to have contracted malaria in New York, or on her way to Chicago; but evidently the case was one of acute septicæmia, although Dr. Cole examined the patient's blood and found it stocked with malarial organisms: for "notwithstanding the administration of 40 to 60 grains daily [of quinine], and the fact that the patient was kept profoundly cinchonized, the chills and fever increased in frequency and severity. Some abscesses occurred in the site of subcutaneous injections. Malarial infection of the blood continued till the death of the patient."

Case 10. March, 1895. Woman with rheumatic symptoms and erythematous nodes. "Examination of the blood discovered small endoglobular bodies, and large amœboid endoglobular bodies containing pigmented particles in active motion." "Under anti-malarial treatment the rheumatoid pains disappeared; the erythematous nodes continued, but were lessened in number; *but the chills and fever were not benefited.*" [Italics mine.—R. N.]

Case 13. A child 3 months old was supposed to have contracted malaria from a nurse. "An examination of her blood [the nurse's] showed malarial infection. Examination of the infant's blood revealed large and a few small endoglobular bodies." "*No fever.*" [Italics mine.—R. N.]

In Case 10 the fact that "the chills and fever" were not benefited by quinine, if they were due to malaria, is incredible; and in Case 13 the statement that the child had "no fever," and yet its blood showed numerous malarial parasites, is also questionable. But the following remarks, quoted from his article, show that he does not agree with the best authorities on malaria. He says: "The proofs of water infection are quite as strong as those of air infection, and there is not a doubt that impure water is responsible for the occurrence of paludal disease with immensely greater frequency than is generally known. . . . Nor is the therapeutic test a reliable guide to diagnosis. . . . The entire absence of fever is a characteristic of many cases. . . . Quinine causes some symptoms to disappear, while others persist; and those showing periodicity may be the ones to persist." Such observations

and the doubtful proof of malaria in the cases he relates, minimize the value of the article.

Such is the evidence coming from American physicians, and certainly it is not sufficient to prove the water-borne theory. Let us now look at that offered by certain doctors in India; three papers by Ross,[19] Rogers,[20] and Hehir[21] may furnish some further proof one way or the other. Ross believes that "malaria must be originally a disease among mosquitoes" (a view brought into more prominence by Manson), and says: "We may conjecture that infection may arise from the bite, or from deposition of the parasite by the mosquito *on* the skin of the patient or in his drinking water." Rogers brings no proof, but states: "In addition to the undoubted effect of drinking water in causing malarial fever, it is also universally admitted that perhaps an even commoner mode of entrance of the organism is through being inhaled into the lungs and so reaching the blood of the capillaries." In Hehir's article we find the following:

"Malaria is mixible with water. It is capable of being carried by currents of water through distances and periods of time altogether undetermined, without losing either its toxic effects or faculty of reproduction. It is possible that this means of conveyance has affected its distributions to continents and islands too widely separated to justify a belief that it was wind-wafted. No observations need be adduced to establish the water-borne habit of the malarial poison, or the positive liability to its toxic effects, when received into the stomach through this medium. These facts have been well understood from the time of Hippocrates. The inhabitants of tracts of marshy country are pretty generally agreed that these waters may produce fevers."

He also thinks that milk left outdoors through the night in open pans may catch sufficient quantities of swamp poison to produce sickness, if exposed where malaria is rife. Daly [*loc. cit.*] also thinks that milk may carry malarial infection, through the cans in which the milk is stored being infected by dirty water in washing them. The ancients may have believed that water could cause malaria, but we cannot accept their statement as proof, nor this one of Hehir's. This evidence which comes from India, where as yet the exact differentiation between typhoid fever and malaria is evidently not generally well understood, or where the discoveries of Laveran are not accepted (ex: Surg.-Gen. Laurie), adds nothing convincing to the water-borne theory.

In Germany malaria is not common, but much has been written on this disease by Germans who have studied it in foreign countries. Shellong,[22] who wrote on the malarial fevers in New Guinea, does not believe in the water-borne theory. His is a careful study and his ideas deserve consideration. Steudel,[23] also writing on foreign fevers, believes in the water-borne theory, and refers, like so many other authors, to Hirsch for confirmation. Hirsch's work, "Handbuch d. Historisch-geographischen Pathologie," was published, however, in 1860 and, though valuable as a reference book, cannot be accepted as an authority on malaria. Leonhardt,[24] in a long and careful study, comes to the conclusion that malaria is not water-borne. He says those who believe it taken in with drinking water cannot prove this, because patients are subject to infection by the atmosphere also. Two out of three Germans, we see, do not believe in the water-borne theory. Let us now review the beliefs of the French and Italian observers on this point.

Taking up the papers first which have appeared in French, we find articles by Verdan,[25] Nicolas,[26] Pepper,[27] and Kelsch and Kiener.[28] These contributions, like the German, are on malarial diseases as seen outside of France. The malarial fevers in Algiers are ascribed by Verdan to the water supply, which is very foul, and to effluvia carried by the winds. The work by Nicolas is a long one, but there is no proper differentiation made between typhoid, malaria and other forms of fever, so that his conclusions are not much to be relied upon. He believes stagnant water to be one of the main sources of origin of malarial fevers ("mais nous en savons assez déjà pour nous convaincre que la stagnation de l'eau est nécessaire pour engendrer la malaria"), but says little about drinking water. However, it may be surmised that he believes also in the water-borne theory, since the supplies of drinking water in the countries of which he writes are often stagnant. He recommends good, pure drinking water, and says, "en ce qui concerne la malaria la preuve semble faite: l'eau d'alimentation n'y est pour rien." But as regards typhoid fever he makes the following statement: "en réalité rien n'est moins prouvé que ces relations de causalité entre l'eau et la fièvre typhoïde en particulier si savamment défendues par des observateurs très recommandables." Such statements diminish the value of his observations.

Pepper, who like Verdan wrote on the fevers in Algiers, concludes his paper, which is not a brief one, with certain aphorisms, from which I quote the following:

1. "Dans une region malarienne, la malaria domine presque toute la pathologie."

2. "La malaria est, selon nous, l'aerotellurisme proteiforme."

3. "L'intoxication se produit durant l'acte de la respiration et atteint principalement le système nerveux."

The first statement is very true, not only in Algiers but in many other countries, practitioners commonly attributing many maladies to malaria as a cause where malaria has actually no causal relation to the diseases in question. His third aphorism shows that he lays no special stress on water as producing malaria.

The final paper by Kelsch and Kiener is also against the water-borne theory. They believe infection is through the lungs, that this at least is the most common method. They think the water-borne theory is not confirmed by facts, and that the cases on the Argo do not support the theory. They say, however, that malaria may arise at sea, and relate one instance which furnishes no stronger proof than that of the Argo. Soldiers in Algeria, they note, who while marching drink foul water, are no more affected with fevers than when in camp using pure water. Towns on heights around the Roman Campagna are free of malaria, although their inhabitants drink water coming from malarious regions. This they bring forward as proof against the water-borne theory and say that foul water simply acts as a force to reduce the resistance of the body to morbid influences.

"Professor Colin, of Val de Grace, who is so well known for his researches on intermittent fever, is inclined to question the production of paroxysmal fevers by marsh water. He cites numerous cases in Algiers and Italy where impure marsh water gave rise to indigestion, diarrhœa and dysentery, but in no case to intermittent fever, and in all his observations he had never met with an instance of such an origin of ague." (Sternberg: Malaria and Malarial Diseases, 1884.) According to these French observers, then, the weight of evidence is against the water-borne theory.

The observations made by Italian physicians are among the most interesting we have. Celli,[29] one of the best students of malarial fever, has written a paper of great value on this subject. It is a short article going to prove that water does not transmit malaria. He gives a chart showing that introduction of new water supply into a certain city did not diminish the number of cases of malaria. He says popular ideas on this matter are fallacious, and notes that persons may live in a malarious place and drink pure water and yet have malaria, while others in the same place drinking foul or "paludal" water do not have malaria. He himself gave water from the Pontine Marshes, which are acknowledged to be very malarious, in large quantities to a certain number of patients who had never had malaria, without its giving birth to this disease in them. Similar evidence is furnished by Salomone-Marino.[30] He experimented in the same manner as Celli, giving 5 to 24 litres daily of paludal water for from 6 to 24 days, and did not succeed in a single instance in producing malaria. He says that those cases of malaria which are suspected to have been caused by infection through the digestive tract are usually grave cases with short periods of incubation. He relates two such cases, which, as the blood was not examined in either case, do not furnish proof of his statement. One patient was very ill and took three weeks to recover (typhoid?). Stagnant water is likely, he thinks, to contain poisonous or noxious bodies which could produce gastro-intestinal symptoms similar to those sometimes caused by malaria. Bonizzardi,[31] without relating any cases, concludes one short article with a number of deductions, from which I take the two following: "Intermittent fever may be contracted as well by drinking water as by air," and "infection by air is far more fatal than by water." Moscato,[32] who has also carefully studied the question of malarial fevers, states, without furnishing any proof, that drinking water as a source of infection has been noted daily by observers: "l'acqua potabile, come sorgente del velino palustre fu riconosciuta anche da Ippocrate; quale verita e stata anche sanzionata dagli osservatori odierni." The statement has been made that malaria may be transported from one infected district to another healthy one by a new water supply, but the proof here is not sufficient, since the turning up of the soil for the pipes may have been the real cause. Most of the Italian observers, it is seen, are opposed to the water-borne theory.

Such is the evidence that exists for and against this theory, and though we cannot say that malaria is never contracted from drinking water, yet the best proof is against this common belief. It is impossible to demonstrate that something never occurs, and we must be satisfied with showing that all the evidence that has been so far collected to confirm the water-borne theory is not of sufficiently exact quality to carry much weight. To prove that malaria is water-borne in a given case demands conditions of a peculiar character: first the patient must never have had malaria, and must live and have lived in a district supposedly quite free of malaria; after drinking a certain supply of water and allowing for the proper period of incubation, the malarial parasite must be found in the patient's blood. In the meantime no soil should have been turned up in the patient's neighborhood, he should not have exposed himself to infection in any way, as by traveling into a malarious country, sleeping out at night, etc. Even if all these demands could be satisfactorily met, there would at the present time still be the proof lacking that the malarial parasite lived in the water. Outside of the human body, we know nothing of the life-history of this parasite. It may be discovered later and found to thrive in water, in which case the probability of frequent infection through drinking water would be strongly enhanced. But it may turn out that it does not live in water for any length of time, and then the possibility of infection through this medium would be greatly diminished. All we can say is that no case among all those recorded as water-borne gives a positive finding of the malarial parasite in the blood, certainly one of the most necessary proofs to show that the patient really had malaria.

To confirm our view we have the definite knowledge that drinking water from malarial places does not produce the fever experimentally. And finally, it is probable that a large number of the cases attributed to malarial infection are not malaria, but rather typhoid, and if not the latter, then due to some auto-intoxication from the intestine. We know how many illnesses starting in with vomiting, headache, chills, diarrhœa, and even blood in the stools, sometimes rapidly ending in death, are due to poisonous articles of food; why then should we not have similar attacks which do in a measure simulate malarial infection, where the most marked symptoms are intestinal, as a result of drinking foul waters, as one of the Italian physicians mentioned above suggests? This does not seem unlikely, and until we know more of the life-history of the malarial parasite outside of the human body it is fair to question the water-borne theory of malaria.

Much light may be thrown on this question in the future by further study on the climatic and telluric conditions underlying the existence of malaria in different places; but as yet, though an enormous amount of study has been given to these very points, the results obtained by the observers do not agree altogether, and we are unable to form any definite conclusions which elucidate the life of this obscure parasite. Above all, in the study of this subject it is absolutely necessary that malaria and typhoid fever should not be confounded, as they have been so often in the past.

In conclusion I would simply add the two following paragraphs from Sternberg's Malaria and Malarial Diseases, which, though written in 1884, are as true to-day as then. He says: "It is well established that enteric fever and certain alvine fluxes may result from the use of contaminated drinking water. And in the case of the former disease, we know that even very great dilution does not destroy the infectious properties of the con-

taminated water, *e. g.* in those cases where it has been used to wash vessels in which milk was placed, and in which typhoid fever has resulted from drinking this.

"That fevers having an intermittent or remittent character may also be produced in the same way cannot be doubted. But we have already seen, in the introduction to the present volume, that enteric fever frequently presents these characters; that in this case the differential diagnosis from malarial fevers presents great difficulties, and that in many parts of the world fevers of this character are attributed, without question, to malaria. We therefore feel inclined to accept the evidence with a great deal of caution. Moreover, we think that considerable weight should be accorded to the negative evidence. It would seem that if this mode of infection occurs at all, it should occur frequently, and in that case that it would be more generally recognized. From what has been said in the introduction, it will be seen that we cannot accept evidence relating to the production of 'malarial diarrhœa,' or 'malarial dysentery,' or 'mountain fever,' or 'continued remittent fever,' or 'typho-malarial fever' in any of its forms."

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Discussion.

Dr. Welch.—It is clear from Dr. Norton's careful analysis of the existing evidence for and against the water-borne theory of malaria, that the weight of evidence and of authority is decidedly opposed to this theory. It is curious how prevalent is the belief in this theory among both the general public and practitioners of medicine, especially in the southern part of this country. As a matter of fact, the only infectious diseases whose germs we have good reason to believe to be conveyed by drinking water, are typhoid fever, Asiatic cholera, and dysentery. To this meagre list may be added indefinite gastro-enteric disturbances resulting from infection or intoxication from drinking water. The numerous claims that various other infectious diseases, including malaria, may result from infection through the drinking water, do not rest at present upon any substantial basis of demonstrated facts, although it must be admitted that some of these claims have not been positively disproved. The criticism of the drinking-water theory of malaria by Celli and Leri, and above all their

convincing experimental investigations of this question, indicate decidedly that drinking water is not a carrier of the malarial organism in a condition capable of producing infection through the alimentary canal under any ordinary conditions. There seem to me to be at present only two theories of the mode of malarial infection which deserve serious consideration, viz. the theory that infection occurs through the air, and the theory that infection is the result of inoculation with the malarial parasite through the agency of mosquitoes and perhaps other suctorial insects. The question whether or not the malarial parasite may live and perhaps multiply in water would of course not be decided by the exclusion of water as a vehicle of infection, for it is possible, even probable from experiments, that infection with the malarial parasite does not occur from the intact alimentary canal, and the mucous membranes of this canal would be the only natural portal of entry for infectious agents in drinking water. The majority of authoritative writers on malaria have advocated the theory of air-borne infection, but we shall probably, since Manson's publication, hear more and more of the inoculation theory.

Dr. REED.—Dr. Norton has kindly referred to a report which I made to the Surgeon-General during August last, upon the etiology of malarial fevers at Washington Barracks and Fort Myer, during a period extending over twenty-six years. Washington Barracks is situated in the midst of marshy lands at an elevation of not more than twenty feet above the Potomac river, and upon soil deposited from this river. The post has had but one supply of water, viz. Potomac river water. During this time malarial fevers have prevailed to a greater or less extent, certain years being exceedingly severe and others comparatively light. I compared the malarial curve of that station with that of Fort Myer, which is 240 feet above the Potomac and on high ground. I was surprised to find such a close agreement between the two. Indeed, in the last eleven years of this investigation the lines ran accurately together. If the line at Washington Barracks fell below that of Fort Myer, it was accounted for by the fact that the garrison had been sent into summer camp during the malarial season. In investigating the water supply at Fort Myer, I ascertained that until 1886 pure spring water had been furnished. During the latter year certain wells were sunk in low marshy soil, from which the garrison was supplied until 1894. It was curious to note that the supply of "marshy water," which was so much condemned by the various medical officers, did not increase the malarial cases; but that, on the contrary, during the six years immediately following the introduction of this supply, the disease was less prevalent than at any other period in the history of the post. In 1894, wells were discontinued and Potomac water introduced. Filters were placed in the men's quarters and, in addition, the filtered water which the officers drank was boiled. Notwithstanding these precautions, the sick-rate from malarial diseases was higher in 1895 than during the previous twenty-three years, and corresponded accurately with the rate at Washington Barracks. All of which proved to my mind that the water supply had nothing to do with the prevalence of malarial diseases, and that their etiology was to be found in the low marshy lands lying along the Potomac river.

Dr. THAYER.—Certainly all experimental evidence points directly against the idea that malarial fever can be conveyed by drinking water, and more than that against the idea of an infection through the alimentary tract. Grassi and Feletti fed patients upon the dew collected from malarious districts, and further upon fresh blood taken from patients suffering from malarial fever, blood which, as is well known, if introduced hypodermically or intravenously, always results in a transference of the infection, without any positive result. Despite the fact that the patients on whom these experiments were made were often debilitated individuals suffering from chronic nervous diseases, etc., yet as they were, in almost all instances, in hospitals or in situations where they were well fed and well cared for, it would be impossible to entirely refute the critic who might suggest that it was the good surroundings which saved the patients from a progressive infection which otherwise would have occurred. Such a critic might suggest that had these individuals been suffering from distinct gastro-intestinal lesions, infection would readily have occurred. In connection with this possibility it is interesting that out of 46 cases of amœbic dysentery which we have had in the Johns Hopkins Hospital, 5, or nearly 11 per cent., have suffered at the same time from malarial infection. May it be possible that the amœbic ulcers were in these instances the portals of entry for the malarial parasite? It must of course be said, upon the other hand, that most of these instances of amœbic dysentery occurred during the malarial season and came from districts where the malarial parasite is doubtless endemic.

Dr. Norton's quotation from the author who thought he had found a large number of endoglobular, hyaline parasites in infant's blood is interesting, inasmuch as any observer who is familiar with examinations of fresh blood will immediately see how the author quoted has deceived himself. The vacuolic-like figures found particularly in the blood of children or infants who are anæmic or suffering from fever of any sort, simulate often very closely the malarial parasite, and always deceive the uninitiated; to the skilled observer, however, they are readily recognizable.

JOURNALS, ETC., ISSUED BY THE JOHNS HOPKINS PRESS OF BALTIMORE.

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THE RADICAL CURE OF HERNIA BY IMPLANTING A SECTION OF STERILIZED SPONGE.

By W. B. Platt, F. R. C. S. (Eng.)

[*Read before the Johns Hopkins Medical Society, January* 4, 1897.]

The radical cure of hernia has long enlisted the attention of surgeons, and a method which is efficient, safe, reasonably simple, sacrificing no important part or function, causing little inconvenience and demanding but a short confinement to bed, has been eagerly sought. To simplify matters, the older surgeons cut the Gordian knot by removing the testicle and cord of the affected side. The presence of the cord at the end of a shallow funnel seemed to be the obstacle to the complete closure of the inguinal canal and rings, the pressure of the intestines acting as a wedge to keep them open.

To remove the cord by transplanting it to another location was the next step of the modern surgeon, and an efficient means of cure. The hernia oftener failed to return than by other methods. Certain drawbacks in this procedure are often noted, such as prolonged confinement to bed and great swelling of the testicle, so great in fact as to throw some doubt upon the future function of that organ, while very large incisions in the abdominal wall, a long cicatrix, a multitude of buried sutures, made the operation more complicated to the surgeon and less agreeable in the end to the patient.

The writer has endeavored to efficiently close the inguinal rings and canal by a more simple procedure, and this can be done. The cases are all too recent to prove the permanency of the cure. He can only say that two cases of intractable hernia have been up and about for several months, and have never worn any truss or support of any kind since getting out of bed.

The idea of implanting a section of sponge in the inguinal canal which should form a firm connective tissue barrier to the descent of the hernia, occurred to the writer while experimenting upon rabbits in the Physiological Laboratory of the Johns Hopkins University in the spring of 1882, and observing the normal retraction of the testicle into the abdominal cavity in these animals.

As inguinal herniæ are not readily produced in animals of this kind, I endeavored to so implant a piece of sponge within the inguinal canal as to retain the testicle in the scrotal pouch. The aseptic technique of that day was so imperfect that suppuration ensued and the sponge came out.

Later, I operated on a number of patients at the Garrett Hospital for Children, by simply removing the sac, suturing the conjoined tendon to Poupart's ligament, and sewing up the inguinal canal.

This succeeded very well in most cases. I have one of these cases here to-night, done five years ago. He has had no return of the hernia and wears no support or truss. The return of the hernia, however, in a case where I had every reason to hope for a permanent cure, led me to try the implantation of a section of sterilized sponge. The result certainly proved one thing, viz. that a piece of sterilized sponge as large as half a dollar could be introduced beneath the internal ring and remain there without causing any irritation, the wound healing over it and remaining healed.

Before this healing took place there was in my first case a small sinus at the upper part of the incision and I feared it might be due to the sponge. It turned out to be due to sutures, and these being removed, the wound healed without further trouble, but after five months the hernia came down. I operated upon this patient a second time, intending to insert a second piece of sponge. To my surprise I found an opening less than one-fourth of an inch in diameter in the location of the external ring. I inserted two small sutures, the wound healing promptly over them. The sponge in this first case was simply *laid* beneath the ring.

In the succeeding three cases the section of sponge was secured to the cord. One description of the operation will suffice for these cases.

An incision about two and one-half inches long is made directly over the inguinal canal, following its course, the lower end extending about half an inch upon the scrotum. If the hernia is down, the operation is so much the easier. The canal is laid freely open. The sac, whether congenital or acquired, is opened in its entire length, the hernial contents reduced, and the sac cut away close to the internal ring, if congenital, while if it is an acquired sac it is cut square off after being ligated as high up as possible, and dropped beneath the ring. In the case of the congenital sac it is often necessary to leave the thin inferior layer *in situ* and adherent to the cord.

Next, a section of fine, firm, surgical sponge is cut about one and one-half inches in diameter and one-eighth of an inch in thickness. This is sterilized without boiling, by immersion in soda solution, washed in sterilized water, treated with permanganate of potash and oxalic acid, and thoroughly washed in sterile normal saline solution. A radial cut is made in the section. The constituents of the cord are gathered together as high up as possible and placed in the center of the section and at a right angle to it. The edges of the cut in the sponge are now slightly overlapped, and two sutures inserted in the sponge to keep it in place on the cord which now pierces its center. The sponge section is spread out beneath the internal ring, and the conjoined tendon sewed firmly by two or more quilted sutures to Poupart's ligament, using a needle in a handle.

The inguinal canal is now snugly sewed up with silk sutures, and over all the skin sewed with silk-worm gut. It is absolutely necessary to use some kind of drainage in the upper angle of the wound, as there is invariably some serous oozing and swelling. This may be removed after 24 hours.

The patient is confined to bed and on his back for three weeks. If the patient is a boy it is well to use the Bradford frame, to keep him from sitting up and twisting about in bed. The skin sutures are removed on the 9th or 10th day, the wound being treated on ordinary aseptic principles.

After three weeks the patient is allowed to sit up in bed a day or two, then sits for two or three days in a chair, after

which he goes about as usual. It is unnecessary to use any support over the site of the operation beyond a light strip of gauze to protect the fresh cicatrix from friction.

In two of these cases where I was obliged to remove the deep silk-worm gut sutures before the patient left the hospital in order to close an obstinate little sinus in the upper angle of the wound, the hernia returned, as might have been expected. The trouble with these sutures may have been due to too great tension, or to a defect in the sterilization of the sutures. In none of the four cases was there any trouble from the sponge. In the two cases mentioned the wound healed promptly on removing the offending sutures. In the two other cases there was no difficulty whatever in securing complete permanent first-intention union, and there has been thus far no return of the hernia.

It is of course premature to say that there may never be a return of the hernia, nor do I know the ultimate fate of the sponge, whether it remains as organized tissue or whether it is absorbed.

From its open structure and the ease with which it is retained in the tissues it is not improbable that blood-vessels pass into it, and that it makes a framework for connective tissue, a close imitation of that barrier which nature places beneath all normal, internal, inguinal rings and which prevents every male from being a subject of hernia.

It is hardly necessary to say that the greatest care must be exercised in the perfect preparation of the sponge section to insure perfect sterilization. I have the pleasure of showing three of the four sponge cases; also one of the older cases, where the rings were simply sewed up and no sponge section used.

ABSTRACT OF CASES.

All the following cases were operated upon at the Garrett Hospital for Children, Baltimore.

1) G. L., æt. 4. Complete inguinal hernia, right side, developed at two years of age, and is not kept up by trusses. Operation August 21, 1895. One deep suture causing a small sinus was removed 55 days after operation. Patient was discharged from the hospital, October 22, 1895, with the wound entirely healed. It has never since reopened. Hernia reappeared 5 months after operation. The return of the hernia doubtless due to the early removal of the suture closing the internal ring.

2) S. J., æt. 8 years. Complete inguinal hernia, right side. Hernia developed when 2 years old. No truss effectually keeps up hernia. Admitted June 9, 1896. Operation June 17, 1896. Congenital variety of sac. Patient was walking about July 26, 1896. Discharged with the wound entirely healed, August 7, 1896. There has been no return of the hernia at the date of this paper.

3) T. W., æt. 12 years. Acquired inguinal hernia, right side. Ruptured since 2 to 3 years of age, and has been kept up with difficulty. Admitted to hospital August 4, 1896. Operation August 19, 1896. Out of bed September 18th. Walking about September 23. Discharged well with wound healed September 25, 1896. Patient is a student at the McDonogh Institute. There has been no return of the hernia up to the date of this paper.

4) G. M., æt. 5 years. Complete inguinal hernia, left side. Hernia appeared at 3 years of age. Trusses do not retain the hernia. Admitted to hospital August 18, 1896. Operation August 26. The wound healed at first. After about a week a small sinus appeared at the upper angle of the incision. This persisted, until on December 10th two deep sutures were removed, after which the wound promptly healed, and patient was discharged well with wound healed, December 24, 1896. Patient was seen January 1, 1897, when the hernia had begun to return, as evidenced by an elastic bulging tumor at the seat of the internal ring.

NOTES.

In Cases 1 and 4, where the hernia returned, it is clear that the soft tissues of the recent operation were subjected to undue strain by the early removal of the most important sutures concerned in the operation, viz. those binding the conjoined tendon to Poupart's ligament. In all four cases the sponge remained without causing any irritation, the incision healing in each case.

I have not had an opportunity in any case to discover the actual condition of the implanted sponge after the lapse of time.

If sponge will thus heal in the depth of a wound, there is no reason why mats of silk or of other animal tissue might not also be used, and sutured firmly in place to close the openings of exit for herniæ.

It is not impossible that the addition of these animal tissue barriers may be the element necessary to make the former operation of suturing the rings and canal without transplanting the cord, a success in the very great majority of cases.

In no case was any support worn after the patient got out of bed, and in all cases the hernia was operated on because of the intractable nature of the hernia, no truss or support keeping the hernial contents in the abdomen, with the exception of No. 3, where a truss proved so great an annoyance that his family desired operation.

DISCUSSION.

Dr. BLOODGOOD.—I cannot say from observation whether the scar tissue formed with the sponge is any stronger than that formed without it. A study of the statistics of operations for hernia will show that sometimes very simple operations will cure hernia in children. In looking over the cases of different operations, such as MacKewen's, Banks' and others, we find very few recurrences in children. We have found it so here, even in the cases where the wound suppurated and healed by granulation. It is interesting to know that a foreign body can be implanted in the tissues without giving rise to any trouble. Our observations here have caused us to think that silk sutures are more apt to cause suppuration than silver wire, and that the ideal suture would be an absorbable one.

It is unfortunate that Dr. Platt has not had an opportunity to operate upon a recurrence in order to see what becomes of the sponge. I would ask him if he thinks the sponge remains permanently and if he has any observations to show that the scar tissue about the sponge is any firmer than ordinary scar tissue.

Dr. PLATT.—I have not yet had a chance to learn the ultimate fate of the sponge, but I think I shall determine it in the case of this boy whose hernia has returned. I have been surprised that his hernia has not returned sooner, for when I was obliged to remove the deep sutures to close the sinus I expected that it would soon recur.

My belief is that the sponge becomes incorporated into the tissues by aid of leucocytes and the blood-vessels which enter its meshes from the adjacent structures, and that when their work is done these probably atrophy, leaving scar-connective tissue. Whether it is stronger and thicker than other scar tissue I do not know, but will take the first opportunity to find out. After the ordinary Banks operation a funnel opening is left, and the cord acts as a guide to further the descent of hernia. My idea is that by putting in the sponge you obviated perhaps the last fault in the stitching operation, and thus get on without transplanting the cord. It is, of course, too soon to draw conclusions as to the final result from these four cases. I shall report again after operating upon this patient.

ADDITIONAL CASES OF DERMATITIS DUE TO THE *x* RAYS

BY T. C. GILCHRIST, M. R. C. S., L. S. A.

During the time which intervened between the correction of the proofs of my article on "A Case of Dermatitis due to *x* Rays" and its publication, there have appeared in the literature a number of quite recent cases which have come under my notice and which could not be incorporated in my paper (*vide* Johns Hopkins Bulletin, February, 1897). I therefore add five more cases which have been recorded in various journals. Dr. Reid's case is of particular interest because a cutaneous lesion was not only produced on the anterior surface of the chest which was exposed to the rays, but also on the posterior aspect of the chest and on an area which he thinks corresponded to the exit of the rays.

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W. Downe: Lancet. 1896. Vol. II, p. 1049. (Abstract.)
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W. Reid: Brit. Med. Journal. 1896. Vol. II, p. 1744.
A. B. Kibbe: New York Medical Journal. January 16, 1897. Vol. LXV. No. 3.
M. Rendu: New York Med. Journal. February 20, 1897. (Abstract from Progrès Médicale. January 30, 1897.)

Recorder.	Duration.	Distribution.	Character of Lesions.	Subjective Symptoms.	Remarks.
Dr. W. Downe.	Eruption appeared 10 days after exposure.	Back of neck and scalp.	Extensive vesication on the back of the neck. The hair fell out over occiput, but the skin remained normal.	At first a sense of pricking heat.	No further observations given.
Dr. Sewell exhibited a similar case at the same society meeting. (See references.)					
Dr. W. Reid (personal).	Eruption appeared in the evening of the same day. Two exposures, varying from 20–90 minutes.	Chest and abdomen.	First an erythema, followed by papules, and later by exfoliation of the epidermis, which exposed a raw, painful, weeping surface 16 days after exposure.	Painful after the weeping surface was formed.	10-inch coil was used 3 inches from the skin.
Dr. A. B. Kibbe.	20 exposures from ½–5 minutes of left hand, and 3 exposures of elbow. Eruption appeared in a day or two.	Both hands and elbow.	First a slight tingling, then a sense of irritation and itching; 10 days later extensive discoloration round elbow. No vesicles appeared.	At first tingling, followed by itching; but there was no sensitiveness or pain.	Arm was covered by the usual clothing, viz. heavy woolen clothing and coat sleeve. Portion of the skin was excised from forearm for histological examination.
Dr. M. Rendu.	Rays applied daily for 55 minutes.	Chest.	After first application of rays intense erythema appeared, followed by vesicles and later by an eschar which did not heal for several weeks.		

THE MALARIAL FEVERS OF BALTIMORE.

An Analysis of 616 cases of Malarial Fever, with Special Reference to the Relations existing between different Types of Haematozoa and different Types of Fever.

BY WILLIAM SIDNEY THAYER, M. D., AND JOHN HEWETSON, M. D.,
Assistants in the Medical Clinic of The Johns Hopkins Hospital.

218 pages, quarto, with plates and charts. Price $2.00, Postage paid.

[Forming part of *The Johns Hopkins Hospital Reports*, Vol. V, 1895.]

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THE JOHNS HOPKINS HOSPITAL BULLETIN,

Volume VIII.

The BULLETIN of the Johns Hopkins Hospital entered upon its eighth volume January 1, 1897. It contains original communications relating to medical, surgical and gynecological topics, reports of dispensary practice, reports from the pathological, anatomical, physiologico-chemical, pharmacological and clinical laboratories, abstracts of papers read before, and of discussions in the various societies connected with the Hospital, reports of lectures and other matters of general interest in the work of the Johns Hopkins Hospital and the Johns Hopkins Medical School.

The subscription price is $1.00 per year. Volume VII, bound in cloth, $1.50.

[Supplement to The Johns Hopkins Hospital Bulletin No. 72, March, 1897.]

In response to inquiries, the following schedule of Special Courses for Graduates in the Johns Hopkins Medical School has been printed.

JOHNS HOPKINS MEDICAL SCHOOL.

Special Courses for Graduates, from May 1 to July 1, 1897.

Hours.	Monday.	Tuesday.	Wednesday.	Thursday.	Friday.	Saturday.
A. M. 8.30	Surgery. Dr. Bloodgood. (Wards.)		Surgery. Dr. Bloodgood. (Wards.)			
9	General Medicine. Dr. Osler. (Ward E.)	Gynecology. Ether Examinations. Dr. Russell.	General Medicine. Dr. Osler. (Ward F.)	Gynecology. Dr. Cullen. (Wards.)	General Medicine. Dr. Osler. (Ward M.)	
9.30	Gynecological Operations. (Private Operating Room.) Dr. Kelly.	Surgical Operations. (Private Operating Room.) Dr. Halsted.	Gynecological Operations. (Private Operating Room.) Dr. Kelly.	Gynecological Operations. (Private Operating Room.) Dr. Russell.	Clinical Lecture. (Amphitheatre.) Dr. Halsted.	Gynecological Operations. (Private Operating Room.) Dr. Kelly.
10-12	Bacteriology. (Lab.) Dr. Carter. (10-12.) Genito-Urinary Surgery. (Disp.) Dr. Gaither. General Medicine. (Disp.)	General Surgery. (Disp.) Dr. Finney. Genito-Urinary Surgery. (Disp.) Dr. Gaither. General Medicine. (Disp.) Nervous Diseases. (Disp.) Dr. Thomas.	Bacteriology. (Lab.) Dr. Carter. (10-12.) General Surgery. (Disp.) Dr. Finney. Genito-Urinary Surgery. (Disp.) Dr. Gaither. General Medicine. (Disp.) Nervous Diseases. (Disp.) Dr. Thomas.	General Surgery. (Disp.) Dr. Finney. Genito-Urinary Surgery. (Disp.) Dr. Gaither. General Medicine. (Disp.) Nervous Diseases. (Disp.) Dr. Thomas.	Bacteriology. (Lab.) Dr. Carter. (10-12.) General Medicine. (Disp.) Nervous Diseases. (Disp.) Dr. Thomas.	General Surgery. (Disp.) Dr. Finney. Genito-Urinary Surgery. (Disp.) Dr. Gaither. General Medicine. (Disp.) Nervous Diseases. (Disp.) Dr. Thomas.
11	Diseases of Children. (Dis.) Dr. Booker. Lecture and Clinic. (Disp.) Dr. Booker.	Diseases of Children. (Dis.) Dr. Booker.	Lecture. (Private Operating Room.) Dr. Kelly.	Diseases of Children. (Dis.) Dr. Booker.	Diseases of Children. (Dis.) Dr. Booker.	Diseases of Children. (Dis.) Dr. Booker.
12	Lecture. Pathology. Dr. Flexner.	Lecture. Bacteriology. Dr. Welch.	Lecture and Clinic. (Amphitheatre.) Dr. Osler.	Lecture. Bacteriology. Dr. Welch.	Lecture and Clinic. (Disp.) Dr. Thomas.	Lecture. Pathology. Dr. Flexner.
P. M. 2	Diseases of Throat and Nose. (Disp.) Dr. Mackenzie. Diseases of Skin. (Disp.) Dr. Lord.	Diseases of Throat and Nose. (Disp.) Dr. Mackenzie. Diseases of Skin. (Disp.) Dr. Gilchrist.	Diseases of Throat and Nose. (Disp.) Dr. Mackenzie. Diseases of Skin. (Disp.) Dr. Lord.	Diseases of Throat and Nose. (Disp.) Dr. Mackenzie. Diseases of Skin. (Disp.) Dr. Gilchrist.	Diseases of Throat and Nose. (Disp.) Dr. Mackenzie. Diseases of Skin. (Disp.) Dr. Lord.	Diseases of Throat and Nose. (Disp.) Dr. Mackenzie. Diseases of Skin. (Disp.) Dr. Gilchrist.
2.30 to 4.30	Eye and Ear. (Disp.) Dr. Randolph. Clinical Microscopy. Dr. Camac. (2.30-4.30.)	Eye and Ear. Disp.) Dr. Theobald. Pathology. (Lab.) Dr. Livingood. (2.30-4.30.)	Eye and Ear. (Disp.) Dr. Randolph.	Eye and Ear. (Disp.) Dr. Theobald. Pathology. (Lab.) Dr. Livingood. (2.30-4.30.)	Eye and Ear. (Disp.) Dr. Randolph. Clinical Microscopy. Dr. Camac. (2.30-4.30.)	Eye and Ear. (Disp.) Dr. Theobald. Pathology. (Lab.) Dr. Livingood. (2.30-4.30.)

Dr. Cullen will give instruction in gynecological pathology, special attention being given to the examination of uterine scrapings.

For particulars apply to the Office of the Medical School.

March 10, 1897.

PROCEEDINGS OF SOCIETIES.

THE JOHNS HOPKINS HOSPITAL MEDICAL SOCIETY.

Meeting of October 19, 1896.

DR. THAYER in the Chair.

Rarer Cases of and Observation on Streptococcus Infection. —DR. BLOODGOOD.

Case I.—The first case is of especial interest because the patient recovered, although on two occasions pure cultures of the streptococcus were obtained from the blood by aspiration of a vein in the arm. In the records of the hospital only one other such observation had been made—that of Dr. Blumer, in a case of broncho-pneumonia.

In all other cases of streptococcus infection which have recovered, from which blood cultures have been taken, negative results have been obtained.

This case also presents the rare condition of a streptococcus infection of the finger, with an abscess situated between the pectoral muscles, but no evidence of any enlargement of the glands in the axilla. The abscess was distinctly localized and no other abscesses developed.

The patient presented to you to-night was admitted into the hospital, January 28, 1896. A history could not be obtained, as the patient spoke only Polish. He appeared about 25 years of age, strong and well nourished. There were two healed wounds covered with a dry scab on the index and middle fingers of the left hand. The fingers and entire arm presented no evidence of inflammation. The axilla was soft and not tender. The skin over the upper part of the chest was slightly red and œdematous. The greater pectoral muscle was more prominent, and beneath it there seemed to be a fluctuating swelling. The aspirating needle withdrew some thin purulent material. Examination of coverslips demonstrated polymorphous leucocytes and cocci in short and long chains. The rectal temperature registered 104°, pulse 80, respiration 24.

An incision was made through the skin and through the greater pectoral muscle in the direction of its fibres, opening a large localized abscess.

Cultures from this abscess and from the blood taken from a vein in the opposite forearm contained in 48 hours numerous colonies of streptococci. Two days later blood cultures also showed colonies of streptococcus; three observations after the fifth day were negative.

The patient had fever for 16 days, the rise of temperature being from 101° to 104° and frequently 105°. Recovery followed. The wound healed and there was no loss of function of the muscles.

The early diagnosis of fluctuation, due to fluid beneath a large muscle, is a very difficult one. The muscle itself, when relaxed, gives to the fingers a sensation of fluctuation. For this reason in this case the aspirating needle was used to confirm the diagnosis of a deep abscess.

Case II.—The second case presented to-night is of interest because of the very high temperature—106.5° rectal—which was associated with the beginning of an attack of erysipelas of the neck and face. The patient recovered. Five weeks before the attack of erysipelas a large mass of suppurating and adherent glands had been removed from the neck. The wound had healed with the exception of the small granulating area. The patient had been walking about for two or three weeks, with no rise of temperature. There were no cases of erysipelas in the wards. The attack began with a chill, and a temperature of 106.5°, taken one hour after the chill. Coverslips made from the granulating area showed cocci in long chains. No erythema could be seen. The patient was a colored man and only very deep redness can be made out. He was immediately transferred to the isolating ward and given a bath for 20 minutes, the temperature of the water being 70° F. The fever was reduced to 103° and did not rise above 104.6° afterwards. Later the skin of the neck and face became very red and œdematous; no abscesses formed. Coverslip and culture demonstrated streptococcus. Two blood cultures were negative.

The only treatment followed was cold sponging and local application of cold cloths to the neck and face.

During the last two years we have made a great number of coverslip examinations from the surfaces of granulating wounds, and also from cases in which there has been an attack of erysipelas. In almost every case in which there has been an infection of the streptococcus of erysipelas we have demonstrated in the coverslips taken from the granulating wound cocci in short and long chains, and in every case cultures and the clinical course have demonstrated the infection to have been due to the streptococcus. In the coverslips from healthy granulating wounds we have never found the cocci in chains. But only a few were studied also by cultures; in these few, only staphylococci grew. These observations have aided us in the early diagnosis of erysipelas.

Death from streptococcus infection has been a very rare event. These organisms do not seem to have with us the virulence which in former years they must have had. In reading Billroth's and others of the older surgeries one is impressed with the large death rate from erysipelas and streptococcus infection of wounds.

There have been two interesting observations, in both of which, although the skin over a large area, including the lines of incision, was the seat of a very intense erysipelas, yet the healing of the wound was not interfered with. Both patients recovered.

The first patient was an old man, from whom a V-shaped piece had been removed from the lower lip for epithelioma, and the entire chain of glands beneath both sides of the jaw excised. Both wounds were closed. An attack of erysipelas began on the second day, starting from the lip and extending over the entire face and neck. The lines of incision healed *per primam*, and there was no deep suppuration.

The second case was a boy, from whom a large mass of inflamed glands had been excised from the axilla. The mass was very large and very adherent to the vein. Both pectoral muscles were divided to aid in the complete removal. During the dissection along the vein a small abscess cavity was opened, infecting the wound. The wound was irrigated with $\frac{1}{1000}$ bichloride, the pectoral muscles sutured and the skin closed. Cultures from the glands demonstrated streptococcus. The

adenitis followed an infection of the finger. 24 hours after the operation an attack of erysipelas set in, beginning in the line of the incision. The erythema covered part of the chest and neck. Coverslips and cultures from a small opening in the line of the incision demonstrated streptococci. The patient recovered; there was no suppuration of the closed wound; there was no impairment of the use of the pectoral muscles.

These two cases demonstrate that the skin over a closed wound may be the seat of erysipelas and yet the healing of the deep wound may not be interfered with.

Dr. FLEXNER.—The case reported by Dr. Bloodgood is of much interest from a theoretical as well as a practical standpoint. Experiments upon animals have led us to consider that the invasion of the blood by pathogenic bacteria, capable of setting up actual septicæmias, takes place a short time only before death, and in the great majority of instances, if not in all, presages a fatal termination. Flügge pointed out that in anthrax infection in the rabbit the bactericidal effect of the blood-serum is lost before this micro-organism is found in the larger blood-vessels. In pneumococcus infection in the same animal the blood invasion occurs a short time only before death.

Now since cultures are so readily made from the circulating blood of human beings we may expect important contributions to our knowledge of the occurrence of pathogenic bacteria in this fluid, and also of the capacity of the human organism to resist and overcome these parasites when they have once entered the blood and perhaps even increased there.

The elimination of the primary focus of infection must be of the greatest importance in these cases, for it is certainly probable that from this site where the parasites are rapidly increasing not a single infection occurs, but a succession of invasions of the blood takes place. Perhaps after all, where recovery occurs, the power of the body fluids and cells to dispose of considerable numbers of the pyogenic cocci is not actually lost, and those present in the blood have entered from without and temporarily, at least, have not increased in the circulating blood. Under these circumstances the removal of the primary focus of infection before the bactericidal effect is lost, would in all probability determine the issue of the case.

We are told by Baumgarten and by Stern that one does not meet in human beings examples of septicæmia comparable to those seen in some of the lower animals. I think there is reason to doubt these statements, for we have seen at autopsy cases of streptococcus and staphylococcus infection in which the blood was crowded with the organisms and wide-spread capillary thrombi of cocci existed in the internal organs.

Much discrepancy has been noted by writers upon the subject of the virulence of streptococci obtained from severe infectious processes in human beings. The results of the cultivation experiments of Marmorek seem to give the explanation of the variations observed. It is an old observation that a minute quantity of the fluid containing streptococci from the inflamed peritoneum might show great virulence and set up a severe and perhaps fatal infection, while the cultures from this source may be quite without pathogenic effects. Marmorek has made it probable that the difference depends upon the unfitness of artificial culture media for preserving the virulence of the streptococcus pyogenes. Through the use of human blood serum as a medium of cultivation he succeeded not alone in retaining the virulence of these micro-organisms for a time, but actually of increasing it, by successive passages through the bodies of animals and recultivation, to a remarkable degree. Finally it is no criterion of the vitality of streptococci to test them upon artificial media, for Marmorek has also shown that cultures apparently dead are still capable of growth when transplanted to his blood serum medium.

Ophthalmoplegia Externa.—Dr. H. WOODS.

On taking charge of the Presbyterian Eye, Ear and Throat Hospital in September, 1895, I found in the ward, D. C., 72 years of age, under treatment for corneal ulcer of the left eye. This, I was told by my friend and associate, Dr. F. M. Chisolm, had followed an operation performed by him during his service in August, for complete left ptosis. A slight iritis developed after he came under my care, but from this as well as from the ulcer he made a good recovery. Corneal opacity is peripheral, and a few pigment pin-point stains are on the capsule. The right eye is centrally blind from choroidal atrophy, but has eccentric vision. It was discovered during this visit to the Hospital that the extrinsic eye muscles were paralyzed. The paralysis is not absolute, for both eyes can move perceptibly up, down and laterally. He left the Hospital before the corneal ulcer was completely healed, returning in March, 1896. He gives the following history:

Prior to 1861, when he enlisted in the Federal army, he had good sight. He claims to have undergone medical examination at Williamsport, Pa., before enlistment. This is important, for the "Surgeon's Certificate of Disability for Discharge," which I have obtained from Washington, is as follows: "The injury to the eyes of said soldier was caused by over-exertion in lifting heavy timber in the year 1857." Signed by Captain.

The Brigade Surgeon's certificate is: "This is a case of ptosis; but having a suspicion that the loss of vision is feigned, the subject is respectfully referred to the Medical Director for decision." The discharge was approved by the Board of Inspectors. This bears date of February 15, 1862. It appears, then, that the only testimony of trouble before enlistment comes from the captain of his company. Against this there are the patient's statement, the nature of the trouble as given by the Brigade Surgeon—ptosis, and the somewhat weighty improbability of a man with double ptosis being enlisted.

To return to the history, as given by the patient. In November, 1861, while marching rapidly early in the morning, he fell, with a number of others, into a stream and was thoroughly drenched. Within an hour afterwards both eyes were closed. He was unable to raise his lids. He was sent to camp, and discharged the following year, as recorded. For several months he was blind, being able to see only by elevating the upper lid. Then some power slowly returned in the right lid. In 1867, Dr. Brown, of Addison, New York, removed a piece of skin from the right upper lid, adding

greatly to his comfort. From '67 to '95 he relied entirely upon his right eye. Left ptosis remained complete. His occupation was gardening. In the early summer of '95 he noticed occasional floating specks before the right eye; "frequently tried to brush away a fly."

One Monday morning in July he went to his gardening as usual, but soon found he could not see clearly, also that he "put his foot where he did not mean to, *on*, instead of *between* his cabbage rows." He held open the left eye to make his way back to the house, and early in August appeared at the Presbyterian Eye and Ear Hospital. A central choroiditis explained his right blindness. Dr. Chisolm operated as stated, to give him use of his left eye, upon which he now depends. The muscular condition, noted partially in September, and more accurately in March, is as follows: Complete ptosis both sides. The operation on the left upper lid has given a larger palpebral opening than that on the right. The right orbicularis is constantly twitching. Though he can close each eye and wink, that power in the orbiculares is diminished is shown by failure to close the lids tightly—as in blepharospasm, for instance—or to resist in the slightest degree forcible separation of the lids. He seems to retain control of the palpebral and to have lost use of the orbital portion of the orbicularis. The eyes themselves are fixed, the right diverging slightly out and up. When told to look in different directions he turns his head. There is, however, such slight motion up, down and laterally as already mentioned. Convergence and, so far as can be ascertained, rotation by the obliques are lost. The intrinsic eye muscles are spared. Each pupil responds to light, possibly a little tardily in contraction to moderate light, dilates under cocaine and contracts to eserine. He reads ordinary print with a convex 3.5 D, and has with this a range of 2 or 3 inches. Distant vision in the left eye is $\frac{15}{100}$, owing, most probably, to pigment deposit on lens capsule. Each papilla is normal. In the right eye there is central choroidal atrophy. Form field, barring the right central scotoma, is normal. This scotoma extends 10 degrees in all directions, except downwards, where it is 20 degrees.

When C. returned to the Hospital in March he asked, "Why, when I want to put my foot on a place, do I put it where it ain't?" This question is at the bottom of some curious things for which an explanation is not easy. With both eyes open he will reach to the right of an object held in front of him, with his right hand, but grasp it correctly with his left. Similarly, he will put his right foot to the right of an object, a narrow ruler, for instance, upon which he is told to step, but place his left foot correctly. With the right eye closed he grasps correctly with either hand and places each foot correctly. If the left eye be closed he either feels for the object or, turning his head for eccentric fixation, reaches it. When he leans far over, getting close to the ruler on the floor, he places his right foot at times correctly. He did not know his eye muscles were affected until told so at the Hospital last September. I asked my friend, Dr. Thomas, to examine the patient, and his report is appended:

"The frontalis muscles are in constant over-action and possess a normal amount of power. The corrugator supercillii acts with a fair amount of power on both sides. The right orbicularis palpebrarum is the seat of frequent momentary contractions; at intervals there can be seen in the left lower eyelid muscular contractions which are synchronous with these. The right orbicularis is able to close the eye completely, but it is much weaker than normal, as the patient is unable to resist even slight traction on either eyelid. The left orbicularis, weaker than the right, is just strong enough to close the eye. The other facial muscles act well and with equal force on the two sides.

"The muscles of mastication and those of the tongue act normally. No disturbance of sensation can be discovered in the face.

"The electrical examination shows that the facial nerve on both sides can be stimulated with a faradic current of moderate strength, the orbicularis palpebrarum taking part in the muscular contraction.

"To direct stimulation the orbicularis on both sides responds to the faradic and galvanic currents, but stronger currents must be used than is usual. The response to the galvanic current is sharp and quick. The right orbicularis responds to currents of less intensity than the left. The other facial muscles act normally.

"The involvement of the orbicularis in association with ophthalmoplegia is of great interest, bearing out, as it does, Mendel's view that this muscle receives nerve-fibres from the nucleus of the third and fourth nerves. Birdsall and Hughlings-Jackson have reported similar cases.

[Signed] H. M. THOMAS."

Dr. THEOBALD.—I do not know of a case that I have met with in which there was a paralysis of the orbicularis associated with general ophthalmoplegia. I have, however, met with several instances where all of the external muscles of both eyes were involved. I have also met with cases where all of the recti muscles were involved without implication of the oblique muscles. These cases were almost invariably of specific origin.

A Simple Contrivance for Effecting Pneumatic Massage of the Tympanal Membrane and Ossicles.—Dr. THEOBALD.

Any apparatus which proves of use in dealing with affections of the middle ear is likely to be of especial value, for the reason that middle ear troubles are very common. Probably more than 75 per cent. of all the cases of impaired hearing which we encounter are due to changes in the middle ear. Deafness from nerve lesions is comparatively rare, forming not more than 2 or 3 per cent. of the cases of deafness met with in hospital and dispensary practice. Middle ear deafness is commonly the result of a chronic catarrhal inflammation, or perhaps of an acute inflammation of a higher grade, with plastic exudation. The changes are of one or two characters; either we have a sclerosis of the articulation of the ossicles, especially of the stapes, where it articulates in the oval window; or there is a formation of bands which attach one ossicle to another, or one ossicle to some part of the wall of the tympanic cavity, in such a way as to interfere with the movement of the ossicles in response to the sound waves. Various devices

have been made to overcome this rigidity or ankylosis of the delicate joints in the middle ear, and especially contrivances which have been described as effecting one form or another of massage. There have been several distinct kinds of massage of the ossicles and tympanum proposed. One is a direct form of massage, pressure massage. The original way in which this was accomplished was by pressure with a delicate probe upon some portion of the malleus, either upon the short or the long process. This produced a certain amount of motion throughout the ossicular chain and in the various joints. More recently electricity has been brought into play in this direction, and one contrivance has been made by which the rapid motion obtainable from the electric current has been made use of to operate a delicate probe which has a spiral spring in it. This probe strikes repeated delicate blows upon the long or short process of the malleus. Another form of massage is designated as phonetic massage; that is, sound is used to produce vibrations of the tympanic membrane and ossicles. Several contrivances have been used for this purpose. One form is the phonograph, in which the sound is carried by tubes to the ear. The phonograph cylinders are cut deeply, so that the sounds are especially sharp and distinct and produce greater mechanical effect.

Pneumatic massage obtained by means of variations in the pressure of the air in the auditory canal is a third method. As far back as 1771 this method of influencing the movement of the ossicles was suggested by Cleland. More recently, this early experiment having been lost sight of, Moos suggested it again. There are several ways in which this has been brought about. The use of the Politzer bag is one method. This forces out the tympanic membrane and moves the delicate joints of the ossicular chain. Another method is with Siegel's pneumatic speculum, by means of which the air in the auditory canal is alternately condensed and rarefied, and thus the tympanal membrane is moved back and forth. One objection to this and other similar contrivances is that the power exerted is too great, and unless they are used with much care, serious injury may be done. If the pressure either outward or inward is too great there is a possibility of rupture of the tympanic membrane, or hemorrhage may be produced. I do not think that marked beneficial results can be claimed for any of these forms of massage, although undoubtedly in some instances they do good, and it is a method which every one should be able to employ in treating ear maladies. One or two things are essential in order that this method may be employed with the hope of doing good. In the first place, the nature of the contrivance should be such that harm is not likely to be done in its use, as is the case when large air bulbs are used. If one uses a large air bag like the Politzer bag, a very little pressure of the hand will produce a marked variation in the density of the air in the auditory canal. Such a contrivance as this it is out of the question to place in the hands of a patient. It is essential in these cases, which are almost always chronic, that the massage should be of such a character that the patient may be able himself to practice it, and this is what I had in mind in contriving this apparatus. The bulb should be so small that one can do no harm in using it. This instrument consists simply of the rubber bulb taken from an eye-dropper, placed upon a small hard rubber tube with a bulbous tip which fits tightly into the auditory canal, rendering it air tight. This can be used in two ways. In the first place, we can compress the bulb before inserting the tube in the ear, and then by letting go the small bulb we rarefy the air and draw the membrane out. On the other hand, we can place it in the ear tightly, and then by squeezing the little bulb we drive the drum membrane in. I direct my patients to reverse the action, at one time drawing the membrane out, at another pressing it in. Hynson & Westcott have made these instruments for me of several different sizes, so that they will fit any ear.

Excision of a Parovarian Cyst without Removal of its Ovary or Tube.—Dr. KELLY.

I will speak this evening of a new step in the conservative treatment of the uterus and its appendages. Before speaking of the subject in hand, I will call attention to a few of the advances we have made in the past few years. We no longer think of taking out the ovaries for enlarged Graafian follicles. If such a follicle is found it is simply exsected and the ovaries sewed up; small follicles may be punctured or treated by the cautery as recommended by Pozzi. Cysts of the corpus luteum may be treated in like manner.

These parovarian cysts (showing pictures) were all removed several years ago and treated radically by removing the tube and ovary as well. I would not treat them so now, as I shall shortly demonstrate. This conservatism extends also to the tubes. Prof. Martin of Berlin has succeeded in saving quite a number of diseased tubes without hurt to the patient and with full restoration to health. Last year I removed from a patient the tube of one side and the ovary of the other, laid the ovary and the tube both down behind the uterus, and the lady, who is the wife of a physician, returned home and has since borne a child. Still graver affections also come under this conservative treatment. We have for some years past been treating accumulations of pus behind the uterus, by puncture, evacuation and simple drainage through the vaginal outlet, even when the pus is in both tubes and ovaries. This has been followed by a large number of perfect recoveries. Occasionally the patient will return and require another drainage operation; the ovaries and tubes can be removed later with greater ease, if necessary. In cases of myomatous uteri I have removed as many as 9 myoma through 7 incisions and had perfect recovery and perfect relief follow. I demonstrated last week the possibility of exsecting as many as 16 myoma without opening the uterine cavity.

The particular new subject along these lines of which I wish to speak to-night is simpler than many of the steps already taken, from a surgical standpoint, and yet, so far as I know, it is novel and is one of the last to follow in the line of conservative effort: that is, the removal of certain parovarian cysts without sacrifice of the correlated ovary and tube. To show how simply this may be done, I pass around specimens of such cysts in which the tubes and ovaries were also removed. It is evident at a glance how simple a matter it would be to remove these parovarian tumors and then to sew up the incision area after the enucleation. The first case I operated upon conservatively was in November, '95. The

patient was suffering a great deal with pelvic pain, and the doctor who had examined her felt two tumors in the pelvis and sent her to me with the statement that both ovaries would have to be removed. I found one cyst the size of a hen's egg before the uterus and another behind it. On opening the abdomen the cysts were both found to be parovarian; they were in the mesosalpinx, and the ovaries were displaced and separated from the tubes by them. I treated the tumors in two different ways, both giving the same result. In one, after ligating the larger vessels a little distance from the tumor, I made an incision through the peritoneum where the vessels were fewest, and I easily shelled the tumor out of its bed and then sewed up the peritoneum with a continuous catgut suture. On the other side I excised a portion of the redundant peritoneum while the circulation was controlled by the finger of an assistant, turned the tumor out and stitched up the wound with catgut. The uterus which had lain in retroflexion was suspended by two silk sutures by my method. Both ovaries were thus left intact. The patient made a perfect recovery, has kept her functions, is normal and regular, and in good health. This, I think, with the other circumstances cited, is one of the last and not least important of our gynecological advances, of particular importance to young women and women in the prime of life.

Dr. WELCH.—How large a proportion of these cases are eligible to this treatment? As one who sees the ordinary parovarian cysts after operation and at autopsy, it is not clear to me how they can be taken out without removing at least the tube. The cyst grows between the layers of the broad ligament (there is no mesosalpinx, the layers are all separated), and the cyst comes up against the Fallopian tube, and, in some cases at least, it would be impossible to dissect it from the tube. The ovary may be quite well preserved, but not unfrequently it is spread out over the surface of the cyst. I suppose Dr. Kelly does not mean that the conservative operation can be done in tumors of that kind, but refers to tumors of only moderate size.

Dr. KELLY.—Yes, the operation is most easily applicable to tumors of medium size, but can also be applied to many of the larger ones. In all of those cases where the cyst is thin walled and more or less pellucid, by slitting the capsule and evacuating the tumor, it can then be drawn out from under the tube, even though the tube is flattened out as shown in this specimen which I pass around. I would exclude from such treatment all those cases in which papillary elements are visible.

Meeting of November 16, 1896.

Dr. THAYER in the Chair.

On the Pathology of Hæmatozoan Infections in Birds.—W. G. MACCALLUM.

Peculiar advantages are offered by the tissues of birds for the study of hæmatozoan infections, in that all stages and degrees of infection may be examined without any difficulty in obtaining the materials. The material for the following observations was furnished in the tissues of the birds whose blood has just been described, and often the extent of the changes in the organs was surprising when contrasted with the apparently good health of the birds.

Kruse, Danilewsky and Labbé have noted in a general way pigmentation of the spleen, liver and bone-marrow, and have observed the enclosure of pigment masses in makrophages in the capillaries of these organs. There is nowhere any mention of inequality in the distribution of the parasites as such in the organs of the body, such as has been described in malaria in the human body giving rise to corresponding symptoms; in no communication on the subject have any symptoms been described occurring in infected birds such as to lead to the suspicion of a definitely localized invasion of organisms, nor have we been able to demonstrate in any of our birds such localization or such symptoms.

As to the effects of the products of the organisms on the tissues we cannot speak definitely. No mention is made in the literature of such effects, but we have observed various degenerated areas in liver and spleen, some of which are explained by concurrent bacterial infections, others apparently by the presence of peculiar ovoid bodies with central nuclear particle, which occur in the cells in groups resembling a segmenting organism, while a third variety is still unexplained.

The invasion of the parasite into the red corpuscle appears to be followed eventually by the breaking down of the corpuscle, from the altered hæmoglobin of which there results a straw-colored pigment. This is seen deposited in considerable masses together with the dark pigment formed directly by the parasite. The process of storing these pigments away from the blood stream is similar to that observed in man. The phagocytic cells of the blood are apparently the cells which we have named Rubinophiles,* and in a lesser degree the eosinophiles, although neither of these is very active in engulfing extruded organisms and pigment. Indeed we have never observed in a fresh slide of blood a single pigmented leucocyte, and, although they have been observed to engulf parasites, they have also been seen to attack the parasite, and after disintegrating it to a great extent, to leave the pigment and wander away. Labbé denies the existence of true phagocytosis, and considers the presence of infected corpuscles within leucocytes to a certain extent accidental.

Danilewsky and Sakharoff describe the occurrence of organisms within leucocytes where they undergo part of their development, but we have observed nothing analogous to this. The hæmopoietic organs are of especial interest, and it is in these that the pigmentation has been noted by European writers. The spleen at autopsy is generally somewhat blackened, and on section shows a network of black lines corresponding to the pulp bands where the bulk of the pigment is deposited. This pigment occurs either in large, clear or palely staining makrophages with single or double vesicular nucleus, or in the swollen endothelial cells of the capillaries. In the spleen the large endothelioid cells of the pulp take on the character of makrophages, but in the liver the pigmented

*Cells with whetstone-shaped granules which take on an eosin stain after long standing, but are colored much more readily by acid fuchsin.

cells are all intracapillary. These large cells are seen to contain masses of yellow pigment, masses of black pigment, scattered grains of black pigment, infected corpuscles, shrunken parasites which at first sight look like small masses of yellow pigment, and debris of broken-down red corpuscles and parasites. The liver shows a less marked pigmentation than the spleen, but the character of the deposit is similar. In the bone-marrow which European authors have found so deeply pigmented we have found almost no pigment and but few organisms. The brain, heart, lungs, kidneys, stomach, intestine, pancreas, thyroid and voluntary muscle show no changes and only the presence of organisms which would be expected from the rich blood supply of these organs.

On the Hæmocytozoa of Birds.—Eugene L. Opie.

The presence of parasites closely resembling the malarial organisms of man, within the red corpuscles of birds, is well known. These organisms have been found in varying abundance in the blood of birds of many parts of Europe, but up to the present time they have not apparently been studied in this country. At the suggestion of Dr. Thayer these intracorpuscular parasites have been studied in the Clinical Laboratory of the Hospital. During the months of June, July, August and September one hundred and twenty-five birds have been examined. Of these the greater number, one hundred, were from the neighborhood of Baltimore. Of eighty English sparrows (Passer domesticus) from this locality, nine were infected with intracorpuscular parasites; of twelve red wing blackbirds (Agelaius phœniceus) six were infected; two swamp sparrows (Melospiza georgiana) and one song sparrow (Melospiza fasciata) contained organisms. Of twenty-five birds, including a variety of species from the neighborhood of Dunnville, near Toronto, Ontario, five were similarly infected, namely, one great horned owl (Bubo virginianus) and four crows (Corvus americanus).

Although the small number of the infected birds makes it impossible to arrive at any general conclusions as to the particular species prone to infection or the local conditions favoring infection, a fairly accurate idea of the character of the parasites present in these localities can be obtained. The organisms found resemble the malarial parasites in that they undergo their whole development within the red corpuscle at the expense of its substance, transforming its hæmoglobin into characteristic pigment granules. Like the malarial organism they reproduce by segmentation, and the young parasites come to occupy other red corpuscles. Unlike the malarial organisms the younger parasites do not exhibit active amœboid movements. Unlike the tertian and quartan malarial organisms they show no cyclic development in great groups of which every organism is in approximately the same phase of development, and hence the length of their cycle of growth cannot be readily determined.

Two types of organism, similar to those observed in European birds, are found. Characteristic of each type is the morphology of the full-grown organism. In the one group an irregularly shaped body, approximately round or polygonal, occupies one end of the nucleated red corpuscle; the nucleus has been displaced into a transverse position in the opposite end. In the other group the full-grown organism is an elongated body which lies curved along one side of the normally situated nucleus. The irregular parasite (Proteosoma, Labbé) segments in the peripheral circulation by a process which is almost identical with that of the malarial organisms. Segmenting forms of the elongate parasite (Halteridium, Labbé) are not found in the blood taken, for example, from the wing. Associated with both types of organism are certain large bodies, apparently degenerate and not destined to segment, characterized particularly by the fact that, unlike the ordinary full-grown organism, they contain almost no material with an affinity for stains as methylene blue, safranin, etc. Organisms which flagellate as do those of malaria are observed with both the irregular and the elongate parasites. With the elongate organism, indeed, owing to the abundance of these forms, the whole process of flagellation can be studied with the greatest facility. Among the irregular parasites present in different birds different varieties are not observed. Among the elongate parasites, however, considerable diversity of form exists in different hosts, but the lack of extended observations makes it impossible to establish distinct species, although in certain cases the morphological differences observed are very striking.

Danilewsky, who first observed intracorpuscular parasites in the blood of birds, believed that these organisms are identical with those of the malarial fevers. Certain subsequent observers have attempted to establish an analogy between the different varieties of the malarial parasite and forms which they find in birds. Others have considered the parasites in birds to be entirely independent of those of man. It seems probable that there are in birds at least two distinct species of intracorpuscular parasites. In almost every detail of structure they very closely resemble the malarial organisms of man, but they are, nevertheless, essentially distinct.

Dr. Osler.—The subject which these gentlemen have presented so clearly has a very definite bearing upon the life history of the malarial parasite in man, as it is quite possible that we may get a clue to the outside history of the parasites by a study of the forms in birds. In 1886, I think, just as I was busily engaged for the first time in the study of the malarial parasite, Mr. MacCallum's father, Dr. MacCallum, of Dunnville, Ontario, sent to the Biological Laboratory in Toronto a goose which was supposed to have malaria. I was extremely skeptical about it, as I knew nothing at that time about the parasites in birds, but to my astonishment there were in large numbers the pigmented intracellular organisms, just such as Mr. Opie has demonstrated to us to-night. They were present in very large numbers, and the spleen of the bird was also large and deeply pigmented.

Dr. L. F. Barker.—I have been much interested in this report of the very careful studies of Messrs. MacCallum and Opie. It may be worth while to emphasize the importance in parasitic diseases of the blood and blood-making organs, of studying not only the appearances of the blood itself by clinical methods, but also the appearances of the blood and tissues in microscopic sections after death. One gets a very one-sided idea of the structure of the malarial parasites and their rela-

tion to the cells, if he studies them only in fresh blood or in smear cover-glass preparations, even when dried and stained. In tissues which have been hardened in alcohol it is very easy in sections stained with hæmatoxylin and eosin or with methylene blue, to make out within the half-grown and full-grown tertian parasites of human malaria a relatively large spherical area which stains usually feebly, though sometimes very distinctly in nuclear dyes. Peripheral to this mass is a thin superficial layer which does not stain at all in ordinary nuclear dyes. In this peripheral non-basophile layer of the parasite is situated the malarial pigment when it is present. The internal stainable mass may be centrally placed, though it is more usual to see it situated somewhat excentrically, so that the peripheral layer of non-stainable substance may vary in thickness in different parts of the organism. The internal mass, which is entirely free from pigment, may stain somewhat unevenly, and occasionally it is possible to make out in it an intensely stained nodule which I have thought may correspond to what has been described in stained smear preparations as the nucleus. It would seem more rational to look upon it as the nucleolus or as an aggregation of the chromatin within the nucleus, though for the decision of this point we must wait for further investigations. I should like to ask Mr. Opie and Mr. MacCallum whether or not they have in their studies or in their reading found any indications of or reference to macro-nuclei or micronuclei in malarial parasites, such as have formed the basis of so much interesting research of late years in the infusoria. It is indeed to be hoped that the large size of the malarial parasites in birds will permit of the unraveling of the finer structure of these curious organisms.

I have been much impressed in studying Mr. MacCallum's specimens of the spleens of these birds, by the enormous masses of malarial parasites to be seen there. At first it seemed almost improbable that the huge brownish masses could consist of malarial parasites. It appeared more likely that they could be explained as forms of blood pigment, but careful study with the oil immersion lens will convince any one that they are in reality aggregations of altered malarial organisms.

Mr. OPIE.—I have seen no references to the macro- and micro-nuclei. Labbé, who studied the question most thoroughly, does not mention such structures.

Dr. WELCH.—This is the first investigation of the kind, as far as I know, in this country. It is extremely interesting, and particularly to us, as it was made here. These have a number of points to distinguish them from the malarial organism, and one of the most striking is the absence of movements. I was interested in the statement that all phases of development occur at the same time in the circulating blood. An idea of Golgi as to the development of organisms in groups in the organs of the body would suggest the localized development in this class of organisms. There is no reference here, however, to groups, but each organism developed separately. It suggests that Golgi's observations might be explained by other suppositions.

Dr. OSLER.—I should like to add a word upon the possibility that in very malarial districts man may harbor the parasites without showing special manifestations. We know that in certain regions all dogs have filaria in the blood, and in many parts of Africa the filaria are present in the natives without causing much trouble. It is quite possible, too, that there may be a minor grade of malarial infection in which the organisms are not present in sufficient numbers to cause fever or the characteristic paroxysms.

Dr. THAYER.—Let me also congratulate Mr. Opie and Mr. MacCallum upon their careful piece of work.

The importance of such studies was impressed upon me several years ago by M. Laveran who expressed his conviction that a careful study of the hæmatozoa of lower animals, and especially of those of birds, which so closely resemble the parasites of human beings, was perhaps more likely to throw light upon some of the dark points in connection with the etiology of malaria than any other branch of research which is at present open to us.

Recent observations appear to show that the parasites of birds and human beings are not identical; they are, however, so similar that it is not improbable that the form in which they exist outside of the body and their method of entrance to the body are likewise not unlike the corresponding phases in the life history of the parasite of human beings.

There are, of course, many more points to be studied with regard to the morphological and biological characteristics of these avian hæmatozoa. In these present observations, for instance, no idea has been formed with regard to the length of the cycle of existence of the parasites. Danilevsky believes that he has been able to distinguish an acute and a chronic form, while Celli divided the parasites of birds into three forms with cycles of existence corresponding to the quartan, tertian, and æstivo-autumnal parasites. Here, however, there has as yet been nothing to point out the exact length of the cycle of existence of each generation of parasites.

Further, it would be advisable to make careful measurements and observations of the temperature in infected birds, observations which, to be sure, have been made by other authors, but with regard to which there is as yet a lack of agreement.

It is also, I think, very important that the intimate structure of the parasites of birds' blood should be carefully followed out; this can be done with much greater facility than in the human being, inasmuch as specimens of tissues can be instantly obtained at any time desired.

But really the most important point, it seems to me, as Bignami has emphasized in a recent paper, is to determine, if possible, the manner in which the parasite exists outside of the body, or more particularly its manner and portal of entry. It may well be possible that careful and continued observations of this sort may bring us nearer the solution of the greatest questions in connection with malarial infection.

With regard to the remark of Prof. Osler concerning the possibility that a human being may harbor the malarial parasites in an active condition for a considerable time without showing distinct clinical symptoms, I must say that the more of the disease one sees, the more reasonable seems the possibility that this condition may exist with relative frequency. In many cases of tertian fever after insufficient doses of quinine, or during attempts at spontaneous recovery, a few parasites may be found in the blood for very con-

siderable periods of time without actual clinical symptoms resulting. Dr. Gamble tells me that in private practice he has found the parasite in a number of instances in the blood of patients complaining of vague symptoms who had not as yet had any actual paroxysm. In these instances it is in every way probable that the parasites had not as yet accumulated in sufficient number to produce a chill. If now in a certain number of instances a single tertian paroxysm may be followed, without treatment, by so extensive a destruction of the organisms that fever does not recur for a week or ten days (a not very infrequent occurrence), theoretically there would seem to be no reason why in certain instances a similar destruction might not occur in groups which have not as yet reached a size sufficient to produce actual manifestations; thus there would result an indefinite prolongation of what one might call the incubation period of the infection. That such conditions exist is, I believe, not only possible but probable, though definite proof has yet to be advanced.

Meeting of December 7, 1896.

Dr. THAYER in the Chair.

Discussion of Dr. Bloch's Paper on "Agglutination of Typhoid Bacilli, etc." [See BULLETIN, Nos. 68-69.]

Dr. FLEXNER.—In this connection I wish to speak of some interesting and highly suggestive results just obtained by Pfeiffer and Kolle, with reference to the possibility of successfully vaccinating human beings against typhoid fever. You may recall that Stern pointed out several years ago that the blood of typhoid fever convalescents contains protective principles similar to those present in the blood of animals rendered immune from experimental typhoid infection. It is probable that these protective substances are those on which the phenomenon of agglutination depends; they are different from the alexines or defensive proteids of the normal blood, which possess little or no power of producing agglutination of bacteria, and are destroyed at a considerably lower temperature. These protective principles are exerted against the bacterial cell, are bactericidal and not anti-toxic in nature; thus they differ from the anti-toxines of diphtheria and tetanus, whose actions are directed against the toxin and not the bacterial cells as such.

Proceeding on the basis of Haffkine's results in vaccinating against cholera, which they accept as conclusive, Pfeiffer and Kolle have in a similar manner injected suspensions of typhoid bacilli subcutaneously beneath the skin of the back into three persons who had never had typhoid fever. The symptoms following the injections were elevation of temperature, chill, malaise; but these quickly passed away without leaving unpleasant effects. Following the cessation of the symptoms they found the blood of the vaccinated persons to contain an amount of the protective substances, measured by the power to protect animals from experimental typhoid infection, in excess of what is found ordinarily in typhoid convalescents. Assuming that there exists a relation between these principles and immunity from typhoid fever, it would appear as if in this simple manner an artificial protection from infection with the typhoid bacilli might be given. Appreciating the difficulties in the way of a conclusive and satisfactory test of this method in ordinary life, they suggest that it be given a trial in outbreaks of typhoid fever in barracks and army corps, where, as is well known, large and destructive epidemics sometimes occur.

Dr. REED.—I have had no experience with the serum test. I have made use, however, of Johnson's modification of Widal's method, and, so far, have examined the blood in thirty-four supposed cases of typhoid fever. When I began with this method—that is, the use of the dried blood—Johnson's paper had not been published, so I had to feel my way, as it were. I began by examining a small quantity of the blood dissolved in distilled water, placing a drop on a cover-slip and then inoculating it with an agar culture. I found the method worked fairly well in certain drops, while in others I had introduced too many bacilli, and, while many clumped, there were highly motile organisms to be seen after the lapse of two or three hours. I had just fixed upon a bouillon culture as the proper medium when Johnson's paper appeared, and being convinced then that I was on the right road, I reviewed my cases with this method. I then had distinct clumping with arrest of motility in cases in which I did not get it when using the solid culture. So far I have examined the blood of thirty-four cases, twenty-eight of which have given me a positive reaction, that is, loss of motility and agglutination of the bacilli. It will be seen that I have met with success in a greater percentage of my cases than Dr. Block has reported to-night. Six cases failed to give the reaction. In two of these I found the æstivo-autumnal parasite. A third case that did not give the reaction was considered to be a severe case of typhoid fever in the Freedman's Hospital. The patient had tympanites and high temperature. I examined the blood with negative result. The patient died the following day, and it was found that there was general peritonitis, having its origin in the appendix. The fourth case was a child who manifested distinct brain symptoms, and the attending physician had not been certain that it was a case of typhoid. Two other cases, one in the third and one in the sixth week, failed to give the reaction. Whether they were real cases of typhoid I do not know. The earliest case in which I have examined the blood was that of a soldier at Fort Myer. Soldiers are generally put to bed early, and the reaction here was quite distinct on the fifth day After I had obtained a positive reaction in twenty-two cases, the particular bouillon that I was using—an old glucose bouillon (because there was no other in the laboratory at the time)—became exhausted and I began to use ordinary bouillon. I tried this latter in two new cases from the Freedman's Hospital, and was surprised to find prompt arrest of motility, but no clumping. For the sake of comparison I took the blood of an undoubted typhoid case and tested it with plain bouillon, and had no clumping until more than an hour had passed. I then tried some of my old glucose bouillon cultures, and to my surprise all three cases gave me good agglutination within fifteen or twenty minutes. I then began to examine my hanging drops, in plain and glucose bouillon, at the end of twenty-four hours, to see if I could distinguish any difference between them, and found that from the margins of the

clumps in plain bouillon the bacilli had always grown out as delicate threads radiating across the field and forming a network, whereas the clumps in glucose bouillon do not show any growth. I have repeated the observation many times with the same result. In the Fort Myer case I got good clumping with the glucose, but not with the plain bouillon. So it has seemed to me that possibly the bacilli grown in the glucose bouillon are somewhat more vulnerable to this peculiar action of typhoid blood.

In order to see whether motility of a culture had anything to do with its agglutination, I examined bouillon cultures at periods of seven, thirteen and sixteen days, and ascertained that the old cultures, in which there is no motility, clump about as promptly as recent ones. I then tried, as a matter of interest, to see whether the dead bacilli would agglutinate, and ascertained that massing of the bacilli took place about as promptly in the dead as in the living cultures. I also observed that blood heated to 60° C. for half an hour acts about as well as the non-heated blood. It occurred to me that by heating the blood to 60° one might destroy its normal germicidal action and still leave behind the peculiar body upon which the clumping depends. I have observed the reaction in dried blood after twenty-six days. I have failed to obtain arrest of motility of the bacilli in normal blood, but in one case, probably latent tuberculosis, there was prompt, distinct partial clumping; the non-agglutinated bacilli, however, remained actively motile at the end of three hours.

Dr. THAYER.—There is one point, it seems to me, which must be carefully looked into before we draw too many conclusions, namely, the length of time during which this bacteriocidal power may remain in the blood after recovery from typhoid fever. In one instance at present in the Hospital the reaction is marked 16 months after the attack of fever. It must be said, however, that there is a typhoid osteomyelitis present which might well be the direct cause of the reaction.

NOTES ON NEW BOOKS.

A Treatise on Surgery, by American Authors. Edited by ROSWELL PARK, A. M., M. D., Professor of Surgery in the Medical Department of the University of Buffalo, etc., etc. Vol. I. General Surgery and Surgical Pathology. 799 pp., 356 engravings, 21 plates in colors. *Lea Brothers & Co., Philadelphia and New York*, 1896.

It was not one of the lightest proofs of the true inspiration of the sacred writers that they have handed down to us such nuggets of truth for all ages as the familiar "Much study is a weariness of the flesh, and of making many books there is no end."

Any volume which steps into the crowded arena of medical literature to-day must expect to be challenged at once to show not only negative virtues, the absence of faults, but some positive and original ones besides, some clear *raison d'être*, or excuse for its very existence. Especially is this true of that realm of medicine known as the surgical, in general, and of American surgery in particular, where the new challenger must win his spurs against such giants of the pliocene as Gross and Agnew, and such hydra-headed antagonists of the pleistocene and contemporary as Ashurst, Dennis, and Keen. Hence the first question to be asked is, does this particular aspirant show proof of such approved superiority or peculiarity in weapons, or skill and training in feats of arms, as to give promise of holding his own in such a contest? We believe that he does in several respects. One of these is that the work is, in the broadest and best sense of the term, American, national, catholic. By this we mean, first of all, that singularly fruitful combination of scientific knowledge and breadth with "Yankee" ingenuity of application and intensely utilitarian tendency, so well typified by its distinguished editor and part-author, which has so rapidly won a world-wide recognition alike for American surgery and American scientific discoveries and mechanical inventions. Our whole inventiveness, progress and enterprise are born of the double fact that the thinker must needs do and the doer is able to think.

Again, in glancing over the list of contributors to the volume, no one, I think, can fail to be impressed with the fact that it is nationally representative in the broadest sense. Names which range from Maine to Louisiana, from Philadelphia to St. Louis, and from Tennessee to Michigan, are certainly ample proof of this, and when we remember that the proudest triumphs of American surgery, as represented by the names McDowell, Batty and Sims, to say nothing of a host of minor ones, had their origin not among the polished consultants of the great centers, but among the sturdy general practitioners of the rural districts of the South and Southwest, we see that this arrangement has not merely its fairness, but a genuine basis of practical scientific advantage to recommend it. At the same time an admirable homogeneity approaching that of the classic surgical monographs is given to the work, both by the careful supervision exercised by the editor and the large proportion of chapters in this volume contributed by him personally, much to the reader's satisfaction and advantage. The first five chapters of the volume covering the domain of surgical pathology are contributed by the editor himself. The subject is presented in a comprehensive and yet concise manner, its most prominent characteristic being the clear-cut division which its author insists upon between hyperæmia and its consequences, and the process of repair on the one hand and of inflammation upon the other. The use of the latter term is limited strictly to those reactions of the tissues which result from the introduction of some noxious irritant, usually of bacterial origin. This limitation possesses decided advantages in point of clearness, both of thought and practice, and it is probably desirable that the use of the word should be so restricted in the future from the surgical standpoint; and we believe that the admirable classification which is based upon it, and which it alone makes possible, will go far to recommend it to the profession. From a biological standpoint, as its author himself would frankly admit, the difference between the two processes is one of degree rather than of kind. But divisions are no longer made, either in pathology or biology, in the old hard-and-fast pre-Darwinian sense, and in actual experience the two classes are so sharply distinguished in their characters, their origins and their results as to be entitled to the writing of true and distinct species.

A somewhat novel feature is the introduction of a chapter upon the pathology of the blood, which, however, abundantly vindicates its right to appear in a surgical treatise, as some excellent diagnostic and prognostic tests are based upon it.

Another instance of the breadth of view which pervades the whole treatment of the subject is the careful discussion of the many factors which, by lowering the resistance of the tissues, may predispose to an attack of surgical infection. With a candor which is rare in the operating surgeon, full credit is given to the extraordinary power on the part of both the leucocytes and fixed cells of the body to ward off infections and to repel invaders of every description; and the story of the "battle of the cells" is given in the most graphic manner. This view of the matter is particularly well developed in what is, to our view, one of the most original and valuable chapters of the volume, that upon "Auto-Infection in Surgical Patients." Here the author impresses with convincing clearness and abundant illustration the too-little recognized truth that any secretion of the body tissues may become a poison to the entire system if its elimination is in any way interfered with, not only medi-

cally but surgically. The surgeon is urged to see to it that his patient is thoroughly aseptic externally and internally, particularly as to the alimentary tube. As the author declares, "There is perhaps no condition which so predisposes to sapræmia, septicæmia, or even pyæmia, as this vague condition of intestinal toxæmia, which nevertheless is so often present." The "hard-and-fastness" of some of the definitions in classification of surgical fevers will challenge decided criticism, but for enabling the subject to be clearly grasped, especially by the student, even this has its practical advantages.

A decided novelty in works upon surgery is found in two excellent chapters by the editor upon "Diseases Common to Man and the Domestic Animals." These fill a gap in the knowledge of the practitioner which is already being widely felt, and will prove among the most attractive chapters of the volume. The pathological paragraphs are particularly suggestive, the biological basis of morbid processes and the individual part played by the various tissue-cells being so vividly developed as to give even tubercle-formation the fascination of a story from life.

The chapter upon Syphilis, while covering the ground well, is so much compressed as to produce in places a style so bold and even dislocated as to seriously interfere with clearness. We very much doubt whether any student would be able to recognize a set of Hutchinson's teeth from the extremely brief (and inaccurate) description upon page 209. Indeed it suggests a doubt as to the clearness of the writer's own conception, and we are not surprised to find it followed by the statement, "The appearance described is often absent in syphilitic patients or may result from other causes." The "wax-work-like" tints of the colored illustrations of this chapter also leave much to be desired.

The pathological suggestions in the chapter upon Rickets are most interesting. One, that the disease is essentially a loss of balance between lactic or some other excrementitious acid, on the one hand, and calcium salts on the other, calls up at once Loew's discovery of serious disturbances in plants on account of a similar imbalance between oxalic acid and the magnesium salts. And we doubt if the essence of the rachitic process has ever been better epitomized than in the description of the layer of osteoid tissue of the epiphyseal lines "which is not cartilage and will not become bone."

In Chapter XIV we find the editor's "creed," which is consistently lived up to throughout the whole of the volume. After speaking of the "artificial and unfortunate separation of surgery from so-called internal medicine," he well declares that "it has been no small part of the benefit resulting from modern teachings that these imaginary boundaries and limitations have been swept away, and one of the lessons which this text-book is intended to inculcate is that broad principles underlie disease-conditions, and that one must appreciate their bearings thoroughly in order to practice either medicine or surgery successfully."

The chapters upon Minor Surgery are clear, concise and well illustrated.

The discussion of Cysts and Tumors is comprehensive and thoroughly up to date, the classification (based largely upon that of Sutton) being particularly commendable for its scientific simplicity and ease of comprehension. Growths are regarded as aberrant or "rebellious" groups of living tissue-cells differing mainly from each other in the particular tissues which they reproduce or imitate and in the perfection of their imitation. We would prefer, however, to transfer epithelioma from the "Tumors of Epithelial Type" to those of "Glandular-tissue Type," as its deadly qualities seem to be solely due to its power of "dipping down" or burrowing into the tissues, in its attempts to imitate the sebaceous, sweat or mucous gland of the surface where it originates.

The chapter upon Surgical Diseases of the Skin, while in most respects admirable, especially in its discussion of dermal tuberculoses, includes a good deal of ground which has already been covered, such as neuroma, myoma, epithelioma, lipoma, etc., and is disfigured throughout by the characteristic "Scabies perpetualis sive septennialis" of our dermatologist brethren, for pompous and sonorous Latin terms of classification, whose dignified sound barely covers their childish meaning. Thus, the common every-day wart is teased under a dissecting microscope with a literary needle and forceps into "Verruca vulgaris, Verruca filiformis and Verruca acuminata"; Cancer of the skin parades as "Carcinoma cutis" with two divisions, "Carcinoma tuberosum" and "Lenticular cancer." It is obvious that potatoes in a bin might be "classified" in precisely the same manner and with as euphonious results.

The chapter upon Diseases of the Lymphatic Vessels, by Dr. F. H. Gerrish, is a most original and valuable contribution to a subject which has never received the attention which its importance merits; probably largely on account of the inherent difficulties of the subject and our imperfect understanding of it.

The remaining chapters of the volume maintain a high average, but present nothing sufficiently characteristic or distinctive to call for special comment.

The illustrations throughout are excellent, with a few exceptions, and a most refreshing feature is the large proportion of them which are entirely new, most of them from original photographs. We feel almost lost, though, without our old familiar friend the "opisthotonos" man (but perhaps he will come to cheer us in the second volume), and grievously miss those numerous and beautiful pictures of surgical instruments, with the maker's name in large black letters upon either the blade or handle.

Even the camera, however, has its little weaknesses, and we are further confirmed by these cuts in our conviction that the photograph is in many cases no more necessarily "true to life" than is the "realism" of modern fiction. In not a few of even this carefully selected series, while the outlines are clear, the details of the central area are so indistinct that the assistance of the title and side-lettering is needed to discriminate between them, while in several even this assistance fails to render them entirely clear, as for instance the cut of varicose ulcers (p. 541) and that of fracture of the patella (p. 741). Most of them, however, are clear, as they are typically illustrative, and will constitute valuable and permanent additions to our surgical picture-gallery.

As a whole, the work is fresh, clear, and practical, covering the ground thoroughly yet briefly, and well arranged for rapid reference, so that it will be of special value to the student and busy practitioner. The pathology is broad, clear and scientific, while the suggestions upon treatment are clear-cut, thoroughly modern and admirably resourceful.

Our only general criticism takes the form of a regret that the editor, in some of his chapters, felt himself so much under the necessity of condensing and systematizing and could not give his thought fuller and freer play. But a chapter or volume which fails by being too short is not far from a decided success, and we have Vol. II to look forward to for consolation. W. H.

A Manual of Syphilis and the Venereal Diseases. By James Nevins Hyde, A. M., M. D., and Frank H. Montgomery, M. D. 1 vol., 618 pages. (*Philadelphia: W. B. Saunders*, 1895.)

For several years there has existed the need of a condensed and at the same time sufficiently comprehensive book on venereal diseases, which would enable medical students and practitioners, at a moderate cost, to obtain the latest opinions as to syphilis, and also to keep up with the advancing bacteriological work which is being done in genito-urinary diseases.

The Manual of Syphilis and the Venereal Diseases, by Hyde and Montgomery, is a book which well answers this purpose. It is extremely attractive in appearance, the printing and illustrations (which are numerous) being all that could be desired. The full-page illustration in colors of "Gonococci in gonorrhœal pus" is a beautiful example of the possibility of enlarging from a photomicrograph.

The chapters on Syphilis are written in a most attractive style,

and condensed so skilfully as to make a thorough review of the whole subject in a comparatively few pages. In their discussion of the treatment of syphilis the authors express some views as regards the inadvisability of using opium in combination with mercury which may not obtain the endorsement of the profession.

The chapter on Chancroid closely follows the articles on this subject in Morrow's System of Genito-Urinary Diseases, and in addition furnishes a valuable table, in parallel columns, for the differential diagnosis of chancre and chancroid.

Cystitis and pyelitis are not given the prominence or attention that these comparatively frequent diseases merit, and no mention is made of the value of the cystoscope as a means of diagnosis in vesical and renal diseases.

The articles on Urethritis Acute and Chronic (including Endoscopy) are especially well written, and give a thorough and scientific presentation of the aubject.

The aim of the authors has been a compilation of the most recent literature and thought on "Syphilis and the Venereal Diseases," rather than to put forward any original work or ideas of their own.

A. B. G.

Transactions of the American Gynecological Society. Vol. 21, for the year 1896. (*Philadelphia: Wm. J. Dornan, Printer*, 1896.)

The Society is to be congratulated upon the almost unanimous tendency to conservatism displayed in its papers and their discussion, which gives a tone of advance, not so prominent in past meetings.

It is not surprising that the paper by Segond, of Paris, on the radical treatment of uterine myomata and inflammatory disease of the tubes and ovaries by vaginal hysterectomy gave rise to a most vigorous discussion and met with many pertinent objections; the consensus of opinion of the American gynecologists seeming strongly against its unqualified acceptance.

Dr. Kelly, in a paper on the treatment of extra-uterine pregnancy, urges strongly the vaginal puncture and drainage in cases ruptured in the early months, but not in the acute stage, and proves most conclusively the value of this conservative method by the cases he cites.

Dr. Henrotin, in a paper on the same subject, dwells on the necessity of immediate abdominal operation at time of rupture, even when the patient is in shock. Now that we have come to recognize that shock is but a term expressing all the symptoms of hemorrhage, Dr. Henrotin's advocacy of immediate interference is sustained.

Dr. Noble's paper on suspensio uteri with reference to its influence on labor deserves special mention. The recent condemnation of the vaginal fixation of the uterus by its originators because of its frequent serious interference with labor greatly increases the value of Dr. Noble's careful study and statistics of the abdominal operation. Unfortunately, the statistics do not represent a uniform method of suturing the uterus to the abdominal wall, and it is difficult to draw accurate conclusions. The two cases of dystocia which he cites in his own experience were true fixations, consequently the suspension method cannot be held responsible for the difficulties which he has described.

The statistics collated by Dr. Byrne with reference to the remote results of operations for cancer of the uterus are most painstaking and creditable, but do not point the way to any advance in treatment. Statistics to be of value must contain a complete picture of every case, and here this paper is found sadly wanting, especially on the laboratory side. In every case suitable for radical operation a microscopic diagnosis should be made before it is undertaken. A failure to do this may lead to most appalling mistakes. After removal, sections should be made through the critical points for microscopic study to determine whether the disease has all been removed. If this were done Dr. Byrne would find that the morbid cell changes beyond the primary growth of which he speaks, were a simple extension of the disease into tissue which appears to the eye and touch unsuspicious. Not until these precautions are taken and the results put into statistical tables will one be able to judge fairly of the remote results of operative treatment of cancer of the uterus. The statistics of to-day are absolutely valueless. Winter, in his most excellent article on the question of recurrences (Zeitschrift für Gynec. und Geburt., Band XXVII, Heft 1), apparently depends entirely upon the macroscopic appearance to define the limits of the disease. One who has studied these tumors microscopically can testify to the utter impossibility of fixing the outermost limits of the disease by the eye or touch. On this account one cannot accept Dr. Byrne's conclusion as to the value of his method of performing vaginal hysterectomy by the cautery. Dr. Halsted has proved, by the astonishing results of his operation for the removal of the breast for cancer, that a wide dissection and removal of the glands are demanded; and the same is true in cancer of the uterus, which can only be successfully removed by the abdominal operation.

W. W. R.

Transactions of the Chicago Pathological Society from October, 1894, to November, 1895. Vol. I. (*Chicago: American Medical Association Press*, 1896.)

This little book contains the transactions of a society which is not wholly pathological, and many of the articles are of medical rather than of pathological interest. The pathological papers, however, are well done, as, for example, "The Contribution to the Study of Malignant Growths in the Lower Animals," by Dr. Field, upon which the paper of Dr. Livingood in a recent BULLETIN is an interesting commentary; the report of cases of Leukemia and of Pernicious Anemia, and the paper on Embolic Abscesses due to the Micrococcus Lanceolatus. The papers on Appendicitis, Nephrorrhaphy for Movable Kidney and Tubal Pregnancy, although not strictly pathological, are of general interest to medical men.

It is to be regretted that the volume is not uniformly printed, which will not seem strange when it is stated that the matter appeared first in the current numbers of a medical journal. If such a system of printing is continued in future, it would add materially to the appearance of subsequent volumes to have successive reports set up in a uniform type. The book deserves a better mechanical appearance.

Medical and Surgical Reports of the Boston City Hospital. Seventh Series. By GEORGE B. SHATTUCK, M. D., W. T. COUNCILMAN, M. D., and HERBERT L. BREWELL, M. D. (*Boston: Published by the Trustees*, 1896.)

It is pleasant to know that the issue of reports by the Boston City Hospital has been resumed, and the volume before us is a worthy successor of those which have preceded. A marked feature of the book is the thoroughness of its report on pathological work. This is notably shown in the valuable study of the Lesions in Selected Autopsies by Prof. Councilman and Dr. Mallory, also in Adeno-Carcinoma of the Pancreas, by Dr. Wright. The preliminary essay on Surgical Morals, by Prof. Cheever, is a new departure in hospital reports and one which can be commended. The paper also on Methods of Routine Treatment in the Surgical Outpatient Department is of great value. Altogether the seventh series is worthy of high praise.

Annual Report of the Supervising General of the Marine Hospital Service of the United States for 1894. (*Washington: Government Printing Office*, 1895.)

In addition to routine reports concerning the operations of the Marine Hospital Service, and full accounts of the various hospitals and quarantine stations under its control, this volume contains much that is interesting to the medical man. Among these may be mentioned detailed histories of the cholera and yellow fever epidemics of 1894, and Prof. Kitasato's Preliminary Notice of the Bubonic Plague in Hongkong, China.

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